THURSDAY, NOVEMBER 22, 1888.

THE OPENING OF THE PASTEUR INSTITUTE.

"WE cannot refrain from expressing some regret that the encouragement of scientific research should be one of the things which they do better in France than among ourselves." With these words, trenchant enough if heeded by those in authority on whose ears they may fall, the *Times* concludes a leader on the inauguration of the Pasteur Institute by the President of the French Republic. Such a ceremony naturally suggests two distinct points for consideration: (1) the object of the institution thus inaugurated; (2) the interest attaching to the ceremony.

The Pasteur Institute is remarkable among all others in being the best form of monument ever erected, and at the same time in its being raised during the life-time of the distinguished man of science, in whose honour and for the furtherance of whose work it was designed. That the debt which the community owes to M. Pasteur will never be paid, nor even adequately acknowledged, needs no insistance; but we may be excused if we dwell upon this point a little, for in the multifarious and different battalions of the workers in the army of science there may well be some whose particular work has not quite brought home to them their obligation to him.

The most remarkable characteristic of M. Pasteur's work, the one which places it on so unique a pedestal, is the fertility of its results in every direction. To have elucidated at once the causation of most forms of fermentation, and the causation of most forms of acute febrile disease (this last leading to the infinitely precious invention by Sir Joseph Lister of antiseptic surgery), is on the chemico-biological side of natural science a feat of as great abstract value and of greater immediately practical worth to the community than any one, or even two, of the greatest epoch-making discoveries of physical science. If it were not for the lamentable consequences of the apathy with which the British public regard science and its contributions to their health and wealth, it would be sadly amusing to read, as anyone may do in even well-founded prints, the lay opinion that M. Pasteur is but a hydrophobia curer, and possibly a slightly more successful one than McGovern, the Irish quack. The flame of popular knowledge of current science always burns most unsteadily, and any sensational wind makes it flare for a short time, and then it sinks almost extinguished. It has thus been with the most recent work of M. Pasteur; and so we find at the inauguration of the Institute the wide subject of the chemico-biology of disease processes was subordinated to the representation of the existing condition of our knowledge of the treatment of rabies.

Although, considering the national importance of the general principles of M. Pasteur's work, this preponderance of attention given to one subject may be regretted, it nevertheless must be admitted that a specific instance is more easily "understanded of the people," and may consequently more energetically drive home the wedges of scientific truth. To M. Grancher was most justly accorded the very agreeable task of expounding in a few simple and unadorned sentences the results of the anti-rabietic treatment of M. Pasteur. Though rabies, or hydrophobia, has always occupied such a special position in the public mind, this has not prevented the application of the general principle of public ingratitude; and we are therefore in no wise surprised to find that the benefactor who arose, and, at his own risk and cost, attempted to remove such an evil, should have been received with calumny and misrepresentation. The consolation afforded by the unerring verdict of time rarely comes—as in the present case it fortunately has to M. Pasteur—before the benefit-conferring Prometheus is past receiving it.

M. Pasteur has always borne the monstrous attacks made upon him with such dignity and composure, that the summary by M. Grancher of the great works suggested by him must have been an intense gratification and recompense.

Our sympathy with his pleasure is unfortunately alloyed with regret that of recent years health has been denied him for the perfect enjoyment of his renown.

The announcement by M. Pasteur in 1885 (the year of the epidemic of rabies in London) that he had not only succeeded in rendering dogs refractory to rabies by means of prophylactic inoculations, but had also with the same material attempted, and apparently successfully, the curative treatment of two human beings, marked the commencement of a widespread application of his now fairly well-known methods.

From the first, M. Pasteur recognized the effect that such an announcement would have upon the public mind, and, in addition to forming a resolution only to treat assured cases of rabies (a resolution he had ultimately to abandon on the grounds of humanity), arranged the facts of his work in such a manner as to provide for complete statistical accuracy in his records.

By his prescience we are thus placed in possession of an overwhelming series of facts relating to persons bitten by rabid animals. He arranged those who came to him under these circumstances into three categories.

In the first (Class A) he placed persons bitten by animals indubitably proved to be rabid by the results of inoculation from the spinal cord into normal animals.

Secondly (Class B.), he grouped together those cases in which the state of the animal, though not tested by experiment, was nevertheless certified to have been rabies by a veterinary surgeon.

Finally, he constructed a third order (Class C.) in which were collected those cases in which, owing to escape, &c., of the dog or animal attacking, no precise information as to its condition could be obtained, but only a presumptive suspicion that it was rabid.

Before we review the figures derived from these three classes of patients, it is important to gauge the character of the statistics of the general mortality from the disease with which they have to be compared. It is only since special attention has been drawn to rabies through M. Pasteur's work that trustworthy statistics have been forthcoming. In former years estimates of various kinds were from time to time prepared, but while some authors took only cases of the most virulent kind, and consequently obtained exceedingly high death-rates among those bitten, others accumulated large numbers of instances the details of which were most imperfectly

ascertained, and the mortality percentages thus deduced consequently utterly untrustworthy. The severest test that could be conceived for genuine criticism of M. Pasteur's method is obviously the comparison of the death-rate in his Class A. with that among persons, not his patients, proved to have been bitten by rabid dogs by the fact of at least one of those attacked by the animal dying of the disease. Such a comparison is now fortunately possible. The probability of rabies following the bite of a rabid dog is now definitely ascertained to be from 15 to 16 per cent. of those attacked.

Now the death-rate in M. Pasteur's Class C. is no more than 1·36 per cent., even including every fatal case—that is, inclusive of those persons who develop the disease during the first fifteen days after the bite. The rigid comparison of these two death-rates may well afford M. Pasteur the satisfaction of feeling that he has saved a number (to be counted by hundreds rather than tens) of his fellow-creatures from the most agonizing of deaths, and an enormous number from the worst of apprehensions.

For general biological science the next most interesting statistics are those which seem to reveal the mode of action of the curative and prophylactic inoculations. M. Pasteur's explanations of the beneficial effects of the material inoculated was that the nerve-tissue contained not only the microbes, the causative factors of the disease, but also their metabolic products, and that these latter by accumulation inhibit the growth and spread of the organisms. If, therefore, these products were injected into the blood-stream in sufficient quantity, he believed that the animal so treated would be protected from the malady. In this country Dr. Wooldridge had already proved experimentally the occurrence of such a process in the case of anthrax or splenic fever. Now the accumulated experience of M. Pasteur's laboratory goes very far to establish this theory for rabies also. Thus in Russia, where rabies is frightfully prevalent by reason of its being endemic among wild (wolves notably) as well as among domestic animals, the figures obtained from the respective inoculation stations are most striking :—

	Odessa death-rate per cent.	Moscow death-rate per cent.	Warsaw death-rate per cent.
1886. "Traitement simple" (*i.e.* small quantities injected)	3·39	8·40	3
1888. "Traitement intensif" (*i.e.* large quantities injected)	0·64	1·60	0[1]

It is abundantly evident from these figures that successful protection is due to the energy and frequency with which inoculations are practised, or, in other words, to the quantity of protective material injected. While we cannot too heartily congratulate M. Pasteur on his triumph in finding a cure for this miserable disease, we feel very glad that, since his work has established the true nature of rabies and its mode of propagation among animals and men, the French authorities have at last awakened to the fact that there is no disease which can be more successfully prevented by legislation. M. Grancher exhibited a chart showing the immediate effect of preventive legislation in reducing the prevalence of the malady in the Department of the Seine. For us, our own experience of the measures whereby the disease was temporarily extirpated from London (though now, of course, reappearing since the relaxation of the restrictions) is so strong that we hope this additional evidence will induce our Privy Council to apply such measures throughout the country; and having thus stamped out the disease in England, prevent by suitable contra-importation measures the re-introduction of the disease.

[1] "Intensif" treatment for last sixteen months—no death.

So much for the work of the Institute as immediately in operation. The special interest of the inauguration ceremony is noteworthy. We have already referred to it as being in part due to the personal monument it establishes to the genius of M. Pasteur, but it has a more particular interest for British national science. It lies in the fact that here we see an institution erected for the national purpose of scientific investigation into the causes of diseases and their mode of prevention. We see, moreover, the head of the Executive Government, in company with the members of his Cabinet, personally giving to the movement his cordial interest and support. It must make us all wonder when our Government will cease to regard the social and political importance of scientific investigations with other than an absolutely ineffective interest.

At present, for scientific investigations of this kind this country and its Government are positively dependent upon the charity of a private laboratory, that of the Brown Institution, the income of which, utterly inadequate, is very imperfectly helped by the defrayal on the part of the Government of simply the immediate expenses of the work done for them. And at the same time we wonder when our Government will remove the disgraceful legislative hindrances to British scientific work. Fnally, we may ask, When shall we see the scientific millennium of an English Ministry taking an immediately personal interest in the welfare and support of such an institution? We can only conclude in the spirit of the words of the *Times* with which this article begins; and hope that, if it is generally appreciated how the lead has been taken from this country by France, at least an effort will be made by those who are responsible for the discredit thus forced on us to remove the blot by organizing a somewhat similar institution in England.

PRACTICAL BOTANY.

A Course of Practical Instruction in Botany. By Prof. F. O. Bower, D.Sc., F.L.S. Part I. Second Edition. (London: Macmillan and Co., 1888.)

THE first edition of Part I. of Profs. Bower and Vines's "Practical Botany" was published in 1885 (see NATURE, vol. xxxii. p. 73); and during the three years that have elapsed the book has become familiar in all botanical laboratories, and has proved an important aid to the work of both teachers and students. This first part deals with the Phanerogams and Pteridophytes. Part II., completing the work, appeared only last year (see NATURE, vol. xxxvii. p. 28), and thus the former part has reached a second edition while its companion volume is

still a new book. In preparing the new edition, Prof. Bower has no longer had the active co-operation of Dr. Vines; though, as explained in the preface, the chapters originally contributed by the latter have been almost entirely embodied in the present work. A short account of the book as it now stands may be of service to the readers of NATURE, for as compared with the former edition it has undergone considerable rearrangement and extension. In both these respects the first three chapters, which are introductory to the study of the types, show important changes. The book begins with a list of apparatus, and of the more ordinary reagents, the mode of preparation of which is described. A more detailed list of reagents is now given in the first of the new appendices at the end of the volume.

Chapter I. deals with the making of preparations, and the adjustment of the microscope. Under both heads the directions are most practical and excellent, and cannot be too strongly commended to the attention of students. This chapter is essentially elementary; and only simple methods, such as are indispensable for all workers, are included. A very clear account of section-cutting is given, and in Fig. 1 a diagram is added in order to explain the meaning of "radial" and "tangential" sections, a point which is often puzzling to beginners. The introduction of diagrams is an important feature of the new edition. Though not very numerous (fifteen in all) they will be found a very useful help. Strictly diagrammatic figures have been used in all cases, in order that the student may not be tempted to make use of the illustrations as substitutes for the objects illustrated. The first chapter concludes with instructions on drawing from the microscope, and on measurement.

Chapter II. contains a number of "practical exercises." In the first of these the microscopic examination of the pulp of an apple serves to make the student acquainted with the general characteristics of vegetable cells.[1] The next exercise is on Spirogyra, and the third on the Fern-prothallus. A reference to the general account of the prothallus at p. 300 would here be useful to the beginner. Next comes the Beet-root, the first object of which sections have to be made. Here the chief osmotic phenomena are studied. Protoplasmic movements are illustrated by Trianea or Hydrocharis, and by Tradescantia. The last exercise (in small print) is on cell-division, as shown in the staminal hairs of Tradescantia. This, we think, would have been better omitted at this place. It is impossible in a few lines to give a satisfactory account of so complicated a process, and inaccuracies can scarcely be avoided. Thus the statement that the nuclear "fibres are ruptured in the equatorial plane" cannot be accepted in the light of our present knowledge.

The third chapter, headed "Micro-chemical Reactions," gives a series of further practical exercises of a more advanced character. These are only in part designed for the beginner, and those which he is intended to work through are indicated by a marginal line. The remaining parts of the chapter are designed to be used for purposes of reference, during the subsequent investigation of the types. The improved account of the chlorophyll-grains (p. 51) and the fuller description of vegetable oils (p. 59) may be especially noticed. Into this chapter an adequate account of the chief facts relating to the division of the cell and nucleus might perhaps have been introduced with advantage, as their satisfactory observation with the help of modern methods is by no means beyond the powers of the more advanced students.

[1] A misprint on p. 27 (line 4) may be noticed, where the word "as" has been omitted.

The introductory portion of the work terminates with some useful remarks on staining, clearing, and permanent mounting (pp. 65-70).

The study of the Dicotyledonous types begins with an examination of the seed and embryo in the Bean, the Cucumber, the Sunflower, the Castor-oil plant, and the Marvel of Peru. In the reviewer's opinion it would have been better to begin at once with the seedling, as the structure of the seed cannot possibly be really understood until the reproductive organs have been investigated. This especially applies to the last example cited, namely, Mirabilis, the description of which will, we fear, be found very puzzling by the student.

The main outlines of the description of types remain as in the former edition. Only a few of the chief alterations need be mentioned. Two excellent diagrams of bundle-systems (after Reinke) are introduced at pp. 79 and 81, while the investigation of the seedling Sunflower by means of successive transverse sections is a most valuable addition to the treatment of the difficult subject of the distribution of vascular bundles. Attention may be called to the remark, on p. 95, that the work on the *young* stem of the Sunflower may with advantage be taken earlier. The structure of the stem before secondary thickening has begun must certainly be understood in order that the subsequent changes may be intelligently studied. Diagrams, after Sachs, are introduced at p. 98 to illustrate the origin of the cambium, and at pp. 100 and 102 to show the arrangement of the cells at the apex of the stem. In the account of the periderm (p. 107) the terminology has been revised, and now agrees with that of De Bary. This subject is now illustrated by diagrams. These are original, and so also are those on p. 122, by which the form of cambial cells is explained.

At p. 137, the intercellular space beneath the stoma is still called the "respiratory cavity." This term, though so generally used, is likely to mislead beginners as to the function of the stomata, and the colourless phrase, "air-chamber," seems preferable. To the account of the structure of the petiole, a description of the pulvinus in the French Bean is now added. The Holly has been substituted for the Cherry-Laurel as the type of a coriaceous bifacial leaf; while, as examples of "iso-bilateral" structure, the phyllodes of Acacia, and the leaves of *Eucalyptus globulus*, are introduced. The aquatic type of leaf is well illustrated by Hippuris, in which the sub-aërial and the submerged leaves are compared. Passing by several minor additions, the valuable new section on the fall of the leaf may be especially noted. An account of hairs and emergences has also been added, in small type.

The work on the root of Dicotyledons has been expanded, and the very clear original diagram of its transverse section, before and after secondary thickening, is likely to be of great service.

The stem of the Monocotyledons is now described in two additional types—the Hyacinth, as a bulbous plant, and Elodea, as an aquatic. The last-named plant is also

used to illustrate the modifications shown by submerged leaves, while the leaf of Iris is introduced as the example, among Monocotyledons, of iso-bilateral construction. In the account of the *root* of Monocotyledons we regret that no mention is made of the exodermis or hypodermal layer. The great importance of this layer as a protective dermal structure to the older roots, especially in plants destitute of periderm, has been sufficiently shown by Olivier and others, and there seems no reason why it should any longer be ignored in elementary teaching. The exodermis is indeed mentioned (under the older, but now somewhat confusing, name of endodermis) in the new section on *aërial* roots (p. 195), but it would be a mistake to suppose that it is by any means confined to roots of this category.

The work on the reproductive organs of Angiosperms has been on the whole but little altered, the naked-eye observation of various typical flowers being the most important addition. In the account of the Blue-bell (p. 203) it would perhaps be well to define the *perianth*, as beginners often fail to see that it corresponds to both calyx and corolla. The Rhododendron is introduced (p. 215) for the study of pollen-tubes. The section on the development of endosperm, and on the continuity of protoplasm between its cells, is new (p. 218).

Going on to the Gymnosperms, we find that the description of the stem of Abies now precedes that of Pinus doubtless on account of the simpler external morphology of the former. In the histological examination of the wood of Pinus we are sorry that the expression "irregularities of structure called bordered pits" has been allowed to stand (p. 230), as it does scant justice to these singularly beautiful organs. The introduction of the leaf and the root of the Yew is a great gain to this part of the anatomical investigation, while the description of the reproductive organs of the same plant is of even greater value.

Among the Pteridophyta, the chapters on Selaginella and Lycopodium have scarcely been altered. It may be pointed out that it is not quite accurate to describe the phloëm in the vascular cylinder of Lycopodium as forming a matrix (p. 268), for the true phloëm is limited to the isolated bands of tissue which alternate with the groups of xylem.

In the account of the homosporous Ferns the most important change is the introduction of Pteris for the minute structure of the vascular bundle. For this purpose it is certainly the best easily accessible type. Several useful diagrams now illustrate this chapter, among which that of the vascular skeleton of the Male Fern, will be especially welcome.

Pilularia is added as a new type, representing the heterosporous Filicineæ. It is certainly well that the student should be made acquainted with this interesting group of plants, and this addition may perhaps be regarded as the most important in the book. The relation of the vascular bundle in Pilularia to that of the true Ferns might have been made somewhat clearer if that type of bundle had been described, in which the two ends, as seen in transverse section, are not completely confluent.

Three appendices and an index have been added to this edition, the index referring to both parts of the book. Appendix I. includes reagents, and the method of preparing them. Appendix II. gives the reactions of the various substances occurring in plants: and Appendix III. is a most useful list of material, with directions for obtaining it. This last appendix is in two divisions, the second of which contains the material required for Part II. (Bryophyta—Thallophyta).

The extensive changes which this volume has undergone have rendered it more than ever an invaluable aid to the study of plants in the laboratory. English students may be congratulated on their good fortune in possessing such a hand-book, and we may confidently hope that the present edition of Prof. Bower's work may render even greater services to scientific education than did its predecessor.

D. H. S.

THE SENSES, INSTINCTS, AND INTELLIGENCE OF ANIMALS.

The Senses, Instincts, and Intelligence of Animals, with Special Reference to Insects. By Sir John Lubbock, Bart., M.P., F.R.S., D.C.L., LL.D. "International Scientific Series," Vol. LXV. (London: Kegan Paul, Trench, and Co., 1888.)

MUCH consideration of the ways of ants has imparted to Sir John Lubbock so large a measure of the wisdom of industry, that even King Solomon himself could scarcely have failed to appreciate the result. The work which has just appeared under the above title may be regarded as a sister volume to the "Ants, Bees, and Wasps," in the same scientific series. Its scope, however, is wider, and, in consequence, its subject-matter is calculated to be of even more interest to the general public, notwithstanding that "Ants, Bees, and Wasps" is already in its ninth edition.

"The Senses, Instincts, and Intelligence of Animals" runs to close upon three hundred pages, of which only about the last fifty are devoted to instincts and intelligence. The book, therefore, is primarily a treatise on the organs of special sense throughout the animal kingdom. As such, it deserves to be regarded as a valuable contribution, not only to the library of the general reader, but also to that of the working biologist. For while, on the one hand, it does not presuppose even the most elementary knowledge on the part of its readers, on the other it constitutes an excellent hand-book of reference to the principal literature on the subject. Of course, in the latter respect it is by no means exhaustive, nor does it profess to be what we understand by a text-book. Nevertheless, it will prove exceedingly useful as a book to be consulted by any naturalist who, having previously worked in other lines, may have occasion to require an index to the more important literature of sense-organs, especially of the Invertebrata. Considerably over one hundred authors are alluded to, and the essay is illustrated by 118 woodcuts, derived from the original memoirs quoted.

A considerable portion of this essay is occupied with an account of the author's own experiments on the special senses of insects, &c., together with replies to criticisms which have been advanced by high authorities in Germany and France, both as against some of his facts and some of his inferences. Without going into particulars, we may

say that in every case these replies appear to us completely satisfactory, and are everywhere rendered in a manner the courtesy of which not many English naturalists could nowadays emulate. But, besides answering criticisms, he has in several cases important criticisms to make. For instance, we have a tolerably full republication of his research upon the colour-sense of Daphnia, whereby he so completely overturned the results previously published by the late M. Paul Bert. The following is a good example of the application of his criticism in another direction:—

"With reference to the power which insects possess of determining form, Plateau has recently made some ingenious experiments. Suppose a room into which the light enters by two equal and similar orifices, and suppose an insect set free at the back of the room, it will at once fly to the light, but the two openings being alike, it will go indifferently to either one or the other. That such is the case Plateau's experiments clearly show, and, moreover, prove that a comparatively small increase in the amount of light will attract the insect to one orifice in preference to the other. It occurred, then, to Plateau to utilize this by varying the form of the opening, so that, the light admitted being equal, the opening on the one side should leave a clear passage, while that on the other should be divided by bars large enough to be easily visible, and sufficiently close to prevent the insect from passing. . . . The insects seem to have gone most often to the trellised opening. M. Plateau concludes that insects do not distinguish differences of form, or can only do so very badly. I confess, however, that these experiments, ingenious as they are, do not seem to me to justify the conclusions which M. Plateau draws from them. Unless the insects had some means of measuring distance (of which we have no clear evidence), they could not tell that even the smaller orifice might not be quite large enough to afford them a free passage. The bars, moreover, would probably appear to them somewhat blurred. Again, they could not possibly tell that the bars really crossed the orifice, and if they were situated an inch or two further off they would constitute no barrier. I have tried some experiments, not yet enough to be conclusive, but which lead me to a different conclusion from that of M. Plateau. I trained wasps to come to a drop of honey placed on paper, and, when the insects had learnt their lesson, changed the form of the paper. . . . It certainly seemed to me that the insect recognized the change."

In the remaining portion of the book, or the portion which deals with "Instinct and Intelligence," we have three chapters. The first is an admirable discussion of one of the most wonderful instincts in the animal kingdom, viz. that of the Sphex stinging only the nerve-centres of the spiders, insects, or caterpillars, which she thus paralyzes without killing, before inclosing them with her progeny, whose food they are afterwards to constitute. Sir John has some good critical remarks to offer on the subject, and also some shrewd speculations upon the possible origin of the instinct. His hypothesis very much resembles that which was arrived at independently by the late Mr. Darwin, and which, therefore, is now in part quoted by Sir John. The quotation runs:—

"I suppose that the sand-wasps originally merely killed their prey by stinging them in many places, and that to sting a certain segment was found by far the most successful method, and was inherited like the tendency of a bull-dog to pin the nose of a bull, or of a ferret to bite the cerebellum. It would not be a very great step in advance to prick the ganglion of its prey only slightly, and thus to give its larvæ fresh meat instead of only dried meat."

Here, by the way, we have an excellent instance of the difficulty which we so often encounter in the domain of instinct, when we relinquish the so-called Lamarckian principle of the inheritance of acquired characters. The hypothesis in question goes upon the supposition that some of the ancestors of the Sphex were intelligent enough to notice the peculiar effects which followed upon stinging insects or caterpillars in the particular regions occupied by nerve-centres, and that, in consequence of being habitually guided by their intelligence to sting in these particular regions, their action became hereditary, *i.e.* instinctive. But if, in accordance with post-Darwinian theory, we relinquish this possible guidance by intelligence, and suppose that the whole of this wonderful instinct was built up by natural selection waiting for congenital (*i.e.* fortuitous) variations in the direction of a propensity to sting, say, the nine nerve-centres of a caterpillar—then it surely becomes inconceivable that such an instinct should ever have been developed at all.

A chapter on the supposed sense of direction among the Social Hymenoptera, and another on his now well-known experiments in teaching a dog the use of written signs, bring to a close one of the most instructive and entertaining of the works which have been produced even by Sir John Lubbock.

GEORGE J. ROMANES.

MASSAGE.

Massage and Allied Methods of Treatment. By Herbert Tibbitts, M.D. (London: J. and A. Churchill, 1888.)

IT is seldom that a medical book of such inferior quality has been issued from the press, and the fact that it has found any purchasers is a striking proof how a catching title and an attractive exterior can still mislead the public. Anyone even slightly acquainted with the subject will at once perceive that the writer, whilst professing to teach massage, has not mastered the first principles of the treatment. His modest refusal to accept the office of "high priest of massage" has, indeed, complete justification.

It is not easy to adopt any method in criticizing a work devoid of all attempt at arrangement, but from the chaos of thought and diction we will select a few samples of what the writer has considered suitable food for the minds of his readers.

At the outset the author attempts to define massage, and with a dim consciousness that he has somehow failed, he plaintively declares that his definition is misty. Out of this verbal fog he never emerges, and as he pursues his erratic course it rapidly thickens around him. He has introduced illustrations and quotations from other writers, and in mercy to his readers also gives references to standard authors, who may be read with advantage. Unfortunately he at times becomes bold even to rashness, and launches out on his own account. A few samples of the inevitable result will suffice. On p. 27, whilst in the midst of giving directions for treating the lower limbs, he intercalates the following sentence: "You then massage the muscles from the waist downwards, working

upwards as before." This has no connection with what has gone before or what follows after. On the next page he says, "for the large and small intestine you massage the lower part" (of the abdomen), having evidently forgotten the position of the transverse colon, which anatomists still believe to be a part of the large intestine. Again, after giving all the less important uses of the saliva, he entirely omits its action in changing the starchy foods into sugar, an omission of which a second-year student would scarcely have been guilty. His readers are left in ignorance of the emulsifying action of the bile on fatty foods, and the pancreas is only considered worthy of mention. In fact the writer, after intimating that the functions of the body could be very well carried on without such an important gland as the spleen, with the modern physiology of which he does not acquaint his readers, leaves us under the impression that the organization of the human body would have been much better planned had Dr. Tibbitts been the designer.

The author claims for a battery he has invented certain qualities, which he declares to be unique, although they are possessed by other machines. He claims for his hospital the honour of being the only one to which a school for massage is attached, totally ignoring what is being done at other institutions. He is the forerunner of Apostoli, and modestly likens himself to Paul and Apollos, he does not say which. "Although Paul planted, Apollos watered," is his misquotation of the Scriptures. He robs Sir James Paget of the honour of a "discovery." Sir James "suggested," but Dr. Tibbitts "originated" afterwards! After claiming on very insufficient grounds to be a forerunner, a discoverer, and a prophet, he finally declares that all the authorities before him were as blind leaders of the blind. Charcot, Russell Reynolds, Hughlings Jackson, Gowers, and such small fry, are all wrong—for has he not looked into all the authorities?—and he now announces in defiance of them the tremendous fact that there is no such thing as hysteria! However, the apparent object of the book has been attained, and the great Holloway must hide his diminished head.

OUR BOOK SHELF.

Rock-forming Minerals. By Frank Rutley, F.G.S., Lecturer on Mineralogy in the Royal School of Mines. With 120 Illustrations. (London: Thomas Murby, 1888.)

THIS book appears to supply a real want among students of that now very popular subject of study, microscopic petrography. Many of the existing text-books, which are for the most part written in German and French, demand a larger acquaintance with the principles of crystallography and physical optics than many students of the subject possess. Mr. Rutley evidently possesses a considerable experience of the wants of students, and is familiar with the kind of difficulties which prove most troublesome to them. With the greatest patience he endeavours to remove these hindrances to their progress, pointing out the different senses in which the same term is sometimes employed, cautioning them against prevalent misunderstandings, and advising them as to the best method of forming just conceptions concerning the abstruse problems with which they have to deal. Very noteworthy and excellent are the numerous drawings, which, though severely diagrammatic rather than pictorial, are admirably suited for their object. The student who follows the advice of the author, and by the aid of cardboard, cork, and pins, constructs a series of models based upon these drawings, will be able to realize the essential peculiarities of the several mineral species in a way that no amount of description will enable him to do. In the general arrangement of this book, Mr. Rutley has followed the same excellent plan as Prof. Rosenbusch in the first volume of his excellent "Mikroskopische Physiographie." The first part of the book, comprising 104 pages, is devoted to general considerations, and the second part (144 pages) to a description of the crystallographic and optical peculiarities of the chief rock-forming minerals, these being grouped according to their system of crystallization. In every part of the book there is evidence of the most painstaking care and conscientious attention to accuracy of detail, and we can heartily recommend the book to those who seek for just such an amount of information on optical principles as will enable them to employ the modern refined methods of petrographical research.

A Text-book of Euclid's Elements for the Use of Schools. Parts I. and II., containing Books I.-VI. By H. S. Hall, M.A., and F. H. Stevens, M.A. (London: Macmillan, 1888.)

WE have here the completion of a work which in its first instalment (Books I. and II.) has already won a considerable amount of favourable notice from teachers. The "end" has "crowned the work" in a similar satisfactory manner; and, without entering into any "odious" comparisons with recent like editions, we consider this to be abreast of the best. Great attention has been paid to the arrangement and composition of the text, and the difficulties which delay beginners have been carefully smoothed and explained. The ordinary proofs have been adhered to as much as possible, and, in the words of the preface, "changes have been adopted only where the old text has been generally found a cause of difficulty."

Alternative proofs are given in many cases, which are less cumbrous than those in vogue already. The subject of proportion has been treated on the system advocated by De Morgan, and here great use has been made of the admirable exposition of it given in the Association's (A.I.G.T.) text-book. The principal propositions have been established in a clear manner, both from the algebraical and geometrical definitions of ratio and proportion, and the distinction between the two modes of treatment is well brought out. The whole of this part forms a good introduction to the sixth book.

The additional feature in the complete treatise is the free use in the third and subsequent books of the signs and abbreviations which are recognized by most teachers, and allowed in the University examinations.

The explanatory matter and additional sections contain all, or nearly all, that is looked for nowadays, and include articles on harmonic section, centres of similarity and similitude, pole and polar, radical axes and transversals. The exercises in the text are well graduated, and should bring out the pupil's acquaintance with, and mastery over, the propositions to which they are appended. More difficult problems are led up to by the solution of typical examples. In conclusion, we need only say the work before us contains all that is needful to a student, who, if he has this, will require no other text-book to become an expert geometer—*i.e.* in so far as outside aid can make one.

A Class-book of Elementary Chemistry. By W. W. Fisher, M.A., F.C.S. (Oxford: Clarendon Press, 1888.)

THE number of elementary books for students of chemistry has increased so greatly during the last ten years, that each new introduction gives rise to a question as to whether the author has justified his position in adding another

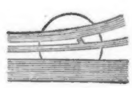
But, however far the supply exceeds the demand, there is always room for what is thoroughly good, especially if it has improvements that its predecessors lack. Though every author is apt to think his pet methods are the very best, and more or less inclined to regard his fads as steps towards perfection, if not indeed its full realization, there are a few who take a sounder view of things, and care nothing for novelty for its own sake. The author of the volume before us has shown that he is one of the few. This book is of sterling value, and will be welcomed by the teacher of elementary chemistry as a guide for his students that he will have pleasure and full confidence in placing in their hands. The volume is well got up, printed in clear type, and illustrated with a sufficient number of excellent diagrams, many from original drawings made by the author. Its 272 pages are not crowded with information or anything else, but the facts included are clearly described in a readable and concise manner. In scope, the book includes the principal non-metallic elements and their chief compounds, followed by the more important metals and their salts. The selection is good and not novel. The periodic law is briefly referred to, and the last eighteen pages are occupied with the chemical physics that it is usually considered well for elementary students to master, such as the relation between specific heat and atomic weight, critical temperature, diffusion of gases, effects of temperature and pressure upon gases, and so on.

LETTERS TO THE EDITOR.

[***The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts intended for this or any other part of*** NATURE. ***No notice is taken of anonymous communications.***]

Alpine Haze.

PROF. TYNDALL has done good service by drawing attention to Alpine haze, and is quite right in adding that it appears in horizontal layers. Such is its common form, but I have also observed a vertical part of it connecting two horizontal striæ rendered conspicuous by concealing portions of a setting sun,



Setting sun observed February 16, 1846, at Garuqqe, in lat. 8° 0'·6, long. 36° 51', at an altitude of 2175 metres.

just as thick boards might do. On another occasion I saw a rough column of it towards the north-west at a supposed distance of three or four kilometres. A few hours later, while I was noting down the phenomenon, a native exclaimed that it had changed its position, and on looking north-west I could see no trace of it, a column similar in size and distance being then in the north-east. It towered above my level on a rolling plain 2300 metres above the ocean. In another place I have seen it at a height of 3600 metres.

Although generally overlooked by meteorologists, this phenomenon has a special name in warm countries. Portuguese call it *neblita*; in Spain it is mentioned as *callina* or *calina*, and Basques name it *lañoa*. Nowhere have I seen it so frequent and thick as in Ethiopia, every different language of that wide region having a special word to express it. The Amara call it *tigag*; the Oromo, or Galla, *qayota*; the Tigray, *taqa*; while old Ethiopic employs the term *qobar*. I have used the latter in my published accounts, because *brume* in French and *haze* in English are generic and not specific names.

Qobar is gray, and of a livid hue when intense, verging sometimes to blackness. The Gascon-speaking population in the Pyrenees call it *bruma terranera*, *i.e.* earth-haze. Its edges are not jagged, like those of clouds, but quite smooth. At Quarata, in 1845, when I was at the level of Lake Tana, the Island of Daga, which rises suddenly 140 metres above the water at an angular height of 16' and a distance of 11·6 miles, was visible only by 4' or 5' of its upper part, the lower 11' or 12' being concealed by *qobar* thicker than usual, and seemingly spread on the lake. I have seen it often on the Red Sea, and sometimes even here in the Bay of Biscay towards the north.

Qobar is the surveyor's foe, and has made me lose several important bearings. It blurs the landscape, diminishes estimated distances, and in Ethiopia is often so thick that no feature of a country is visible beyond the space of a mile.

Fifteen years ago I published in my "Physique du Globe" all that I know about *qobar*. In Germany it goes by the name of *Heiderauch*, or by six other words all ending in "*rauch*." Ethiopians also compare it to but distinguish it from smoke. When commenting on chapter x. of Exodus, their native professors say that the darkness mentioned in verse **22** was an intense *qobar*, and go on to explain that the light enjoyed by the Children of Israel is fully borne out by the fact of *qobar* being sometimes prevalent in one place, yet absent in its neighbourhood. I have noted several instances of this partial occurrence. Without quoting them, I may mention that, according to my working hypothesis, *qobar* is only dry air, visible because in large quantity. On the other hand, astronomers well know that very moist air is the most transparent.

Natives are swarthy in countries where *qobar* abounds. Does it darken man's skin? At all events it is worth while to draw some hundred litres of it through suitable reagents. Chemists could thus test Kaemtz's notion that it is always smoke.

ANTOINE D'ABBADIE (de l'Institut).

Abbadia, Hendaye, November 10.

P.S.—I forgot to mention that, after crossing the three layers shown by the figure, the setting sun crossed two other layers, and finally disappeared behind the lower stratum of *qobar*, then 3° or 4° above the horizon.

Rankine's Modification of Newton's Investigation of the Velocity of Sound in any Substance.

PROF. EVERETT'S letter (November 8, p. 31) calls attention to a difficulty which is apparently felt by students over the attempted elementary method of deducing the general expression for the velocity of sound given in Maxwell's "Heat." Advanced students need feel no difficulty of the kind, because they arrive at it by another path; but inasmuch as the Rankine method seems the easiest available to intermediate students, it is desirable as a matter of pedagogy to put it in its simplest form; and so I venture to quote here the plan I have for some time adopted.

First lead up to the subject by considering the velocity of a hump on a stretched string. Explain the plan of imagining the string to move along at the same pace as the hump, but in an opposite direction, so as to keep the hump stationary in space, obtaining the velocity necessary to do this by equating the normal compound of the tension to the centrifugal force—

$$T \cdot \frac{ds}{r} = \frac{\lambda ds \cdot u^2}{r}, \quad \text{or } u = \sqrt{\frac{T}{\lambda}},$$

where T is the tension, and λ is the linear density of the string; and then actually show the experiment—running a light loose flexible endless cord on a pulley, and making a hump on it. The tension in a loose whirled endless cord free from gravity being that due to the centrifugal force only, viz.—

$$T = \frac{\lambda ds \cdot v^2}{r} \cdot \frac{r}{ds} = \lambda v^2,$$

it follows that $v = u$, and so the keeping of the hump still is automatic, except for a slight interference by the weight of cord hanging below the hump. This interference being less and less notable as the hump is initially made nearer the bottom of the loop of cord.

Next explain, and illustrate by moving diagrams, the simple harmonic motion of the particles of a medium conveying sound-vibrations.

Then proceed to consider a longitudinal pulse travelling along a substance contained in a tube of unit area, and imagine a wind of the substance blowing through the tube in the opposite direction with such a velocity, U, as just to keep the pulse stationary in space.

Except for an unessential disturbance due to friction, the pressure all through the tube is uniform so far as the wind-motion is concerned.

Erect across the tube a couple of imaginary partitions, and watch the substance streaming past them. The state of the substance at either partition, whatever it may be at one instant, remains permanently the same always; hence the mass of substance inclosed between the two must remain permanently the same—for it cannot be always steadily increasing—and therefore the mass of matter flowing through any one plane is constant.

In future attend to one of the planes only, and call the density of the substance at this place ρ. The plane may be at or near a condensation, it may be at or near a rarefaction, or, again, it may be where the substance has its ordinary density; whatever the state of the substance there, the same it remains. The longitudinal-pulse motion of the particles of substance (which has previously been illustrated and discussed at length) is superposed upon the wind-motion; and if we followed any one particle along the stream we should see it simply oscillating with a simple harmonic motion—

$$x = a \sin nt, \quad \text{or} \quad v = an \cos nt,$$

having at any instant the velocity v.

But we are not going to follow a single particle down stream; we are contemplating a procession of particles as they successively pass the fixed partition, and at the instant of passage they are all in a definite phase of their motion—they all have the same definite velocity, v, as they pass, in addition to their general wind-velocity, U. The vibratory velocity v may be in the same direction as U, or it may be in the opposite direction; it may have any value between $\pm na$, of course. So the resultant velocity of each particle as it passes the fixed partition is algebraically $U + v$. This represents the length of cylinder of substance passing through the partition per second; and, since the partition is of unit area, the mass of substance flowing past it per second is

$$m = (U + v)\rho, \text{ and is constant} \quad . \quad . \quad . \quad . \quad (1)$$

This of itself is an interesting result; for it shows that at the middle of condensations, where ρ is a maximum, v must have its greatest negative value; and the particles are therefore all in full swing back against the wind (*i.e.* travelling with the sound-pulse) at the middle of every condensation. At a rarefaction, v has its greatest positive value, and the particles are swinging with the wind (against the sound-pulse). Only at half-way places, where the density of the substance has its average or undisturbed value, are the particles quiescent as regards the sound-pulse.

Next consider the dynamics of the matter, and the force which must act to vary the motion of the particles.

If the pressure were the same on either side the partition, there could be no change of velocity for the particles as they pass. The change of velocity, $d(U + v)$, or dv, must be due to a difference of pressure existing on either side the partition; and if the slope of pressure $\frac{dp}{dx}$ is positive, the pressure is greater on the lee-side of the partition than on the windward side, and so the acceleration $\frac{dv}{dt}$ will be negative. Hence, equating the force acting and the momentum generated by it per second,—

$$dp = - m dv \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (2)$$

This equation, along with equation (1), solves the problem, and determines the velocity U.

Differentiating (1)—

$$(U + v)d\rho + \rho dv = 0.$$

Rewriting (2) by help of (1)—

$$(U + v)\rho dv = - dp.$$

Substituting for ρdv from one of these into the other, we get—

$$(U + v)^2 = \frac{dp}{d\rho} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (3)$$

This equation shows that $\frac{dp}{d\rho}$ is by no means constant all through the substance. It is greatest wherever v has its maximum positive value—that is, at the centre of every condensation; it is least at the centre of every rarefaction; it has an average value in the undisturbed portions of the medium, and it is there equal to U^2.

Hence we learn that the value of U is determined by calculating the ordinary value of $\frac{dp}{d\rho}$ for the medium in its uncompressed and unrarefied state.

So—

$$U = \sqrt{\frac{dp}{d\rho}} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (4)$$

This is the velocity with which the substance must flow through the tube in order to keep the sound-pulse stationary: this, therefore, is the velocity of sound in it.

The result is general, and applies to all substances. But for gases it may be written more explicitly by help of their characteristic equation $\frac{p}{\rho} = RT$, and their adiabatic condition $p \propto \rho^\gamma$: viz.—

$$U = \sqrt{\gamma RT} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (5)$$

T here means the undisturbed temperature of the gas, but if one chooses to allow the equation to follow the fluctuations of temperature ($\pm t$) adiabatically produced in the condensations and rarefactions, one must write the more general form—

$$(U + v)^2 = kR(T \pm t) \quad . \quad . \quad . \quad . \quad . \quad . \quad (6)$$

which gives us the relation between fluctuation of temperature and vibrational velocity. We may also write it—

$$\rho^2(T \pm t) = \text{const.} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (7)$$

which shows the connection between the elevation or depression of temperature, and the density, at any part of a sound-wave.

Speaking as a teacher, I believe one reason why we fail to make things clear is, because we are often in too big a hurry. One's natural tendency is to give such an investigation—as this, for instance, in Maxwell's "Heat"—in a few lines on the blackboard, taking perhaps half an hour or less over it, and forgetting that it embodies in concentrated form a great deal of difficult thought, though the actual mathematics may be simple. Gradually I am learning not thus to scamper over the ground, but to lead up to a thing in two or three or even more lectures, and then to devote a whole hour to the thing itself. By this means, students may ultimately be got to grip and feel the thing as a whole, instead of having to ascend step by step to it; but it is hopeless to expect them to thus grasp it straight off; and even if it were possible, it would not be really desirable for various reasons. The attempt to hurry them into the comprehension of difficulties leads them, I believe, into a vague notion that everything is hazy and half unintelligible. The best thing we can do for them is to get them to see some few things luminously, so that they may not feel inclined to rest satisfied with half-knowledge in other instances.

OLIVER J. LODGE.

November 12.

P.S.—Since writing the above, I have referred to Prof. Everett's note A in Deschanel, and have found it excellent, like all his notes; he happens to have employed just the same means as the above for obtaining equation (1), but for the latter part I prefer my statement. I trust no one will imagine that the above contains anything more than a way of putting things to students.

The slip of a wrong sign in Maxwell I had not distinctly noticed, but the simplest statement of it seems to be that in obtaining the second equation he has put, for the change of velocity of each particle as it passes a plane, du instead of $d(U - u)$; that is, the change of absolute instead of relative velocity.—O. J. L.

A Simple Dynamo.

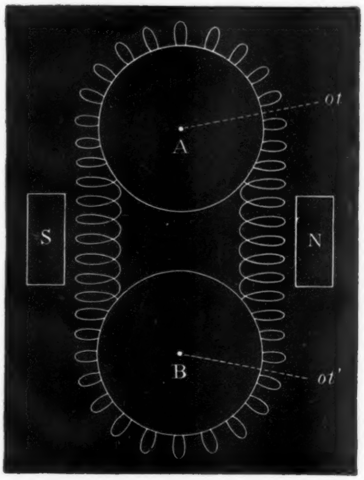
I VENTURE to send you a brief description of a simple electro-magnetic instrument which I have recently devised for illustrating the principle of the Gramme ring.

Two pulleys, A, B, having semicircular grooves, are mounted, as shown in the figure, on a piece of board; round the two wheels is stretched a continuous coil of copper wire; a horse-shoe magnet is placed with its poles close to the vertical parts of the coil; the wheels are connected to the terminals t t': when the wheel A is rotated the whole coil moves, and a steady current is at once generated, which flows from terminal to terminal when they are

connected together, the direction of the current depending on the direction of rotation.

It will be noticed that, since the coil and wheels are always in contact, no undulations are produced as when brushes come in contact with successive ends of coils, as in the ordinary dynamo.

When the instrument is placed in circuit with a sensitive galvanometer, the rotation being constant, no variation in the current can be detected, even when the motion is very slow. The coil when arranged with one wheel at A, and a mercury contact at B, will revolve when a current is sent through it, becoming in this case a motor. If an iron chain, or an elastic band of iron such as a measuring tape, be placed inside the ring coil, it then becomes a



distorted Gramme ring, the wheels taking the place of the brushes, the way in which the current is produced being the same. If coils approaching N produce a current upwards, then those which are leaving N produce one downwards. The same takes place on the other side; coils leaving the S pole produce a current upwards, while those which approach it produce a current downwards; both of the ascending currents, being in the same direction, go to the wheel A, while both of the descending currents, being in an opposite direction, go to the wheel B.

The first coil made was of copper wire. Phosphor bronze wire answers better, being less easily distorted.

FREDERICK J. SMITH.

Trinity College, Oxford, November 12.

The Use of Rotifera.

CAN any of your readers inform me whether Rotifers are of any use in removing decaying organic matter, as Infusoria do?

C. L.

ON THE MECHANICAL CONDITIONS OF A SWARM OF METEORITES.[1]

I.

MR. LOCKYER writes in his interesting paper on meteorites[2] as follows:—

"The brighter lines in spiral nebulæ, and in those in which a rotation has been set up, are in all probability due to streams of meteorites with irregular motions out of the main streams, in which the collisions would be almost *nil*. It has already been suggested by Prof. G. Darwin (NATURE, vol. xxxi. p. 25)—using the gaseous hypothesis—that in such nebulæ 'the great mass of the gas is non-luminous, the luminosity being an evidence of condensation along lines of low velocity according to a well-known hydrodynamical law. From this point of view the visible nebula may be regarded as a luminous diagram of its own stream-lines.'"

The whole of Mr. Lockyer's paper, and especially this passage in it, leads me to make a suggestion for the reconciliation of two apparently divergent theories of the origin of planetary systems.

The nebular hypothesis depends essentially on the idea that the primitive nebula is a rotating mass of fluid, which at successive epochs becomes unstable from excess of rotation, and sheds a ring from the equatorial region.

The researches of Roche[1] (apparently but little known in this country) have imparted to this theory a precision which was wanting in Laplace's original exposition, and have rendered the explanation of the origin of the planets more perfect.

But notwithstanding the high probability that some theory of the kind is true, the acceptance of the nebular hypothesis presents great difficulties.

Sir William Thomson long ago expressed to me his opinion that the most probable origin of the planets was through a gradual accretion of meteoric matter, and the researches of Mr. Lockyer afford actual evidence in favour of the abundancy of meteorites in space.

But the very essence of the nebular hypothesis is the conception of fluid pressure, since without it the idea of a figure of equilibrium becomes inapplicable. Now, at first sight, the meteoric condition of matter seems absolutely inconsistent with a fluid pressure exercised by one part of the system on another. We thus seem driven either to the absolute rejection of the nebular hypothesis, or to deny that the meteoric condition was the immediate antecedent of the sun and planets. M. Faye has taken the former course, and accepts as a necessary consequence the formulation of a succession of events quite different from that of the nebular hypothesis. I cannot myself find that his theory is an improvement on that of Laplace, except in regard to the adoption of meteorites, for he has lost the conception of the figure of equilibrium of a rotating mass of fluid.

The object of this paper is to point out that by a certain interpretation of the meteoric theory we may obtain a reconciliation of these two orders of ideas, and may hold that the origin of stellar and planetary systems is meteoric, whilst retaining the conception of fluid pressure.

According to the kinetic theory of gases, fluid pressure is the average result of the impacts of molecules. If we imagine the molecules magnified until of the size of meteorites, their impacts will still, on a coarser scale, give a quasi-fluid pressure. I suggest, then, that the fluid pressure essential to the nebular hypothesis is in fact the resultant of countless impacts of meteorites.

The problems of hydrodynamics could hardly be attacked with success, if we were forced to start from the beginning and to consider the cannonade of molecules. But when once satisfied that the kinetic theory will give us a gas, which, in a space containing some millions of molecules, obeys all the laws of an ideal non-molecular gas filling all space, we may put the molecules out of sight and treat the gas as a plenum.

In the same way the difficulty of tracing the impacts of meteorites in detail is insuperable, but if we can find that such impacts give rise to a quasi-fluid pressure on a large scale, we may be able to trace out many results by treating an ideal plenum. Laplace's hypothesis implies such a plenum, and it is here maintained that this plenum is merely the idealization of the impacts of meteorites.

As a bare suggestion, this view is worth but little, for its acceptance or rejection must turn entirely on numerical values, which can only be obtained by the consideration

[1] Abstract of a Paper read before the Royal Society on November 15 by Prof. G. H. Darwin, F.R.S.

[2] NATURE, November 17, 1887. The paper itself is in the Roy. Soc. Proc., November 15, 1887 (No. 259, p. 117).

[1] *Montpellier Acad. Sci. Mém.*

of some actual system. It is obvious that the solar system is the only one about which we have sufficient knowledge to afford a basis for discussion. The paper, of which this is an abstract, is accordingly devoted to a consideration of the mechanics of a swarm of meteorites, with special numerical application to the solar system.

When two meteoric stones meet with planetary velocity, the stress between them during impact must generally be such that the limits of true elasticity are exceeded, and it may be urged that a kinetic theory is inapplicable unless the colliding particles are highly elastic. It may, however, I think, be shown that the very greatness of the velocities will impart what virtually amounts to an elasticity of a high order of perfection.

It appears, *a priori*, probable that when two meteorites clash, a portion of the solid matter of each is volatilized, and Mr. Lockyer considers the spectroscopic evidence conclusive that it is so. There is no doubt enough energy liberated on impact to volatilize the whole of both bodies, but only a small portion of each stone will undergo this change. A numerical example is given in the paper to show the enormous amount of energy with which we are dealing. It must necessarily be obscure as to how a small mass of solid matter *can* take up a very large amount of energy in a small fraction of a second, but spectroscopic evidence seems to show that it does so; and if so, we have what is virtually a violent explosive introduced between the two stones.

In a direct collision each stone is probably shattered into fragments, like the splashes of lead when a bullet hits an iron target. But direct collision must be a comparatively rare event. In glancing collisions the velocity of neither body is wholly arrested, the concentration of energy is not so enormous (although probably still sufficient to effect volatilization), and since the stones rub past one another, more time is allowed for the matter round the point of contact to take up the energy; thus the whole process of collision is much more intelligible. The nearest terrestrial analogy is when a cannon-ball rebounds from the sea. In glancing collisions fracture will probably not be very frequent. It must, however, be admitted that on collision the deflection of path is rarely a very large angle. But a succession of glancing collisions would be capable of reversing the path, and thus the kinetic theory of meteorites may be taken as not differing materially from that of gases.

From these arguments it is probable that, when two meteorites meet, they attain an effective elasticity of a high order of perfection; but there is of course some loss of energy at each collision.

Perhaps the most serious difficulty in the whole theory arises from the fractures which must often occur. If they happen with great frequency, it would seem as if the whole swarm of meteorites would degrade into dust. We know, however, that meteorites of considerable size fall upon the earth, and, unless Mr. Lockyer has misinterpreted the spectroscopic evidence, the nebulæ do now consist of meteorites. Hence it would seem as if fracture was not of very frequent occurrence. It is easy to see that if two bodies meet with a given velocity the chance of fracture is much greater if they are large; and it is possible that the process of breaking up will go on only until a certain size, dependent on the velocity of agitation, is reached, and will then become comparatively unimportant.

When the volatilized gases cool they will condense into a metallic rain, and this may fuse with old meteorites whose surfaces are molten. A meteorite in that condition will certainly also pick up dust. Thus there are processes in action tending to counteract subdivision by fracture and volatilization. The mean size of meteorites probably depends on the balance between these opposite tendencies. If this is so, there will be some fractures, and some fusions, but the mean mass will change very slowly with the mean kinetic energy of agitation. This view is at any rate adopted in the paper as a working hypothesis. It was not, however, possible to take account of fracture and fusion in the mathematical investigation, but the meteorites are treated as being of invariable mass.

The velocity with which the meteorites move is derived from their fall from a great distance towards a centre of aggregation. In other words, the potential energy of their mutual attraction when widely dispersed becomes converted, at least partially, into kinetic energy. When the condensation of a swarm is just beginning, the mass of the aggregation towards which the meteorites fall is small, and thus the new bodies arrive at the aggregation with small velocity. Hence, initially, the kinetic energy is small, and the volume of the sphere within which hydrostatic ideas are (if anywhere) applicable is also small. As more and more meteorites fall in, that volume is enlarged, and the velocity with which they reach the aggregation is increased. Finally the supply of meteorites in that part of space begins to fail, and the imperfect elasticity of the colliding bodies brings about a gradual contraction of the swarm. I do not now attempt to trace the whole history of a swarm; but the object of the paper is to examine its mechanical condition at an epoch when the supply of meteorites from outside has ceased, and when the velocities of agitation and distribution of meteorites in space have arranged themselves into a sub-permanent condition, only affected by secular changes. This examination will enable us to understand, at least roughly, the secular change as the swarm contracts, and will throw light on other questions.

The foundation for the mathematical investigation in the paper is the hypothesis that a number of meteorites which were ultimately to coalesce, so as to form the sun and planets, have fallen together from a condition of wide dispersion, and form a swarm in which collisions are frequent.

For the sake of simplicity, the bodies are treated as spherical, and in the first instance as being of uniform size.

It is assumed provisionally that the kinetic theory of gases may be applied for the determination of the distribution of the meteorites in space. No account being taken of the rotation of the system, the meteorites will be arranged in concentric spherical layers of equal density of distribution, and the quasi-gas, whose molecules are meteorites, being compressible, the density will be greater towards the centre of the swarm. The elasticity of a gas depends on the kinetic energy of agitation of its molecules, and therefore in order to determine the law of density in the swarm we must know the distribution of kinetic energy of agitation.

It is assumed that when the system comes under our notice, uniformity of distribution of energy has been attained throughout a central sphere, which is surrounded by a layer of meteorites with that distribution of kinetic energy which, in a gas, corresponds to convective equilibrium, and with continuity of density and velocity of agitation at the sphere of separation. Since in a gas in convective equilibrium the law connecting pressure and density is that which holds when the gas is contained in a vessel impermeable to heat, such an arrangement of gas has been called by M. Ritter (*Annalen der Physik und Chemie*, vol. xvi., 1882, p. 166) an isothermal-adiabatic sphere, and the same term is adopted here as applicable to a swarm of meteorites. The justifiability of these assumptions will be considered later.

The first problem which presents itself, then, is the equilibrium of an isothermal sphere of gas under its own gravitation. The law of density is determined in the paper, but it will here suffice to remark that, if a given mass be inclosed in an envelope of given radius, there is a minimum temperature (or energy of agitation) at which isothermal equilibrium is possible. The minimum energy

of agitation is found to be such that the mean square of velocity of the meteorites is almost exactly $\frac{6}{5}$ of the square of the velocity of a satellite grazing the surface of the sphere in a circular orbit.

As indicated above, it is supposed that in the meteor-swarm the rigid envelope, bounding the isothermal sphere, is replaced by a layer or atmosphere in convective equilibrium. The law of density in the adiabatic layer is determined in the paper, and it appears that when the isothermal sphere has minimum temperature the mass of the adiabatic atmosphere is a minimum relatively to that of the isothermal sphere. Numerical calculation shows, in fact, that the isothermal sphere cannot amount in mass to more than 46 per cent. of the mass of the whole isothermal-adiabatic sphere, and that the limit of the adiabatic atmosphere is at a distance equal to 2·786 times the radius of the isothermal sphere.[1]

It is also proved that the total energy, existing in the form of energy of agitation, is exactly one-half of the potential energy lost in the concentration of the matter from a condition of infinite dispersion. This result is brought about by a continual transfer of energy from a molar to a molecular form, for a portion of the kinetic energy of a meteorite is constantly being transferred into the form of thermal energy in the volatilized gases generated on collision. The thermal energy is then lost by radiation.

It is impossible as yet to sum up all the considerations which go to justify the assumption of the isothermal-adiabatic arrangement, but it is clear that uniformity of kinetic energy must be principally brought about by a process of diffusion. It is therefore interesting to consider what amount of inequality in the kinetic energy would have to be smoothed away.

The arrangement of density in the isothermal-adiabatic sphere being given, it is easy to compute what the kinetic energy would be at any part of the swarm, if each meteorite fell from infinity to the neighbourhood where we find it, and there retained all the velocity due to such fall. The variation of the square of this velocity gives an indication of the amount of kinetic energy which has to be degraded by conversion into heat and distributed by diffusion, in the attainment of uniformity. This may be called "the theoretical value of the kinetic energy." It appears that in the swarm, this square of velocity rises from zero at the centre of the swarm to a maximum which is attained nearly half-way through the adiabatic layer, and then diminishes. It is found that the variations of this theoretical value are inconsiderable throughout the greater part of the range. From this it follows that there must be diffusion of kinetic energy from without inwards, and considerations of the same kind show that when a planet consolidates there must be a cooling of the middle strata both outwards and inwards.

We must now consider the nature of the criterion which determines whether the hydrostatic treatment of a meteor-swarm is permissible.

The hydrodynamical treatment of an ideal plenum of gas leads to the same result as the kinetic theory with regard to any phenomenon involving purely a mass, when that mass is a large multiple of the mass of a molecule; to any phenomenon involving purely a length, when the cube of that length contains a large number of molecules; and to any phenomenon involving purely a time, when that time is a large multiple of the mean interval between collisions. Again, any velocity to be justly deduced from hydrodynamical principles must be expressible as the edge of a cube containing many molecules passed over in a time containing many collisions of a single molecule; and a similar statement must hold of any other function of mass, length, and time.

Beyond these limits we must go back to the kinetic

[1] This is one of the results established by M. Ritter in a series of papers in the *Annalen der Physik und Chemie* from 1878 onwards.

theory itself, and in using it care must be taken that enough molecules are considered at once to impart statistical constancy to their properties.

There are limits, then, to the hydrodynamical treatment of gases, and the like must hold of the parallel treatment of meteorites.

The principal question involved in the nebular hypothesis seems to be the stability of a rotating mass of gas; but unfortunately this has remained up to now an untouched field of mathematical research. We can only judge of probable results from the investigations which have been made concerning the stability of a rotating mass of liquid. Now it appears that the instability of a rotating mass of liquid first enters through the graver modes of gravitational oscillation. In the case of a rotating spheroid of revolution the gravest mode of oscillation is an elliptic deformation, and its period does not differ much from that of a satellite which revolves round the spheroid so as to graze its surface. Hence, assuming for the moment that a kinetic theory of liquids had been formulated, we should not be justified in applying the hydrodynamical method to this discussion of stability, unless the periodic time of such a satellite were a large multiple of the analogue of the mean free time of a molecule of liquid.

Carrying, then, this conclusion on to the kinetic theory of meteorites, it seems probable that hydrodynamical treatment must be inapplicable for the discussion of such a theory as the meteoric-nebular hypothesis, unless a similar relation holds good.

These considerations, although of a vague character, will afford a criterion of the applicability of hydrodynamics to the kind of problem suggested by the nebular hypothesis. And certain criteria suggested by this line of thought are found in the paper; they give a measure of the degree of curvature of the average path pursued by a meteorite between two collisions.

After these preliminary investigations, we have to consider what kind of meeting of two meteorites will amount to an "encounter" within the meaning of the kinetic theory.

Is it possible, in fact, that two meteorites can considerably bend their paths under the influence of gravitation, when they pass near one another? This question is considered in the paper, and it is shown that unless the bodies have the dimensions of small planets, the mutual gravitational influence is insensible. Hence, nothing short of absolute impact is to be considered an encounter in the kinetic theory, and what is called the radius of "the sphere of action" is simply the distance between the centres of a pair when they graze, and is therefore the sum of the radii of a pair, or, if of uniform size, the diameter of one of them.

(*To be continued.*)

SOME CURIOUS PROPERTIES OF METALS AND ALLOYS.[1]

THE lecture consisted mainly of experimental demonstrations of the changes induced in metals, either by slight variations in the treatment to which they are subjected or by rendering them impure by the addition of small quantities of metals or metalloids.

Prof. Austen began by pointing out that for centuries the early metallurgists investigated the action of exceedingly small quantities of matter upon masses of metal; and he said that, strange as it may seem, the promulgation, in 1803, of Dalton's atomic theory threw a flood of light upon chemical phenomena, but cast into the shade such investigations as those of Bergman, which dealt with influences

[1] Abstract of a Lecture delivered by Prof. W. Chandler Roberts-Austen, F.R.S., at the Royal Institution, on May 11, 1888.

of "traces" upon masses, and the authority of Berthollet was not sufficient to save them from neglect. In this eventful year for science, 1803, the latter published his essay on chemical statics, in which he stated, as a fundamental proposition, that in comparing the action of bodies on each other, which depends "upon their affinities and mutual proportions, the mass of each has to be considered" (English edition, by M Farrell, M.D., 1804, p. 5). His views were successfully contested by Proust, but, as Lothar Meyer says, the influence on chemistry of the rejection of Berthollet's views was remarkable:—"All phenomena which could not be attributed to fixed atomic proportions were set aside as not truly chemical, and were neglected. Thus chemists forsook the bridge by which Berthollet had sought to unite the sister sciences, physics and chemistry." Fortunately, however, in this country there was one chemist who had followed up the line of work indicated by the early metallurgists, for in 1803, the same year as that in which both Berthollet's essay and Dalton's atomic theory were published, Charles Hatchett (Phil. Trans., vol. xciii. p. 43, 1803) communicated to the Royal Society the results of a research which he had conducted, with the assistance of Cavendish, in order to ascertain "the chemical effects produced on gold by different metallic substances when employed in certain" (often very small) "proportions as alloys."

Allusion was then made to the evidence of the passage of metals into allotropic states, and it was shown that, although the importance of the isomeric and allotropic states was abundantly recognized in organic chemistry, it had been much neglected in the case of metals. Special attention was then devoted to the works of Joule and Lyon Playfair, who showed, in 1846, that metals in different allotropic states possessed different atomic volumes, and the lecturer then proceeded to the consideration of the work of Matthiessen, who, in 1860, was led to the view that in certain cases when metals were alloyed they passed into allotropic states, probably the most important generalization which has as yet been made in connection with the molecular constitution of alloys

Instances of allotropy in pure metals were then shown to the audience, such, for example, as Bolley's lead, which oxidizes readily in air; Schützenberger's copper; Fritsche's tin, which fell to powder when exposed to an exceptionally cold winter; Gore's antimony; Graham's palladium; and allotropic nickel. It was further shown that metals could be obtained in chemically active states under the following conditions:—Joule proved that when iron is released from its amalgam by distilling away the mercury the metallic iron takes fire on exposure to air, and is therefore clearly different from ordinary iron, and is, in fact, an allotropic form of iron. Moissan (*Comptes rendus*, vol. lxxxviii. p. 180, 1879) has shown that similar effects are produced in the case of chromium and manganese, cobalt, and nickel, when released from their amalgams with mercury.

Evidence is not wanting of allotropy in metals released from solid alloys, as well as from fluid amalgams with mercury. Certain alloys may be viewed as solidified solutions, and when such bodies are treated with a suitable solvent, usually an acid, it often happens that one constituent metal is dissolved, and the other released in an insoluble form. Reference was then made to a new alloy of potassium and gold, containing about 10 per cent. of the precious metal. If a fragment of this alloy be thrown upon water, the potassium takes fire, decomposes the water, and the gold is released as a black powder: there is a form of this black or dark-brown gold which appears to be an allotropic modification of gold, as it combines with water to form auric hydride. If this dark gold be heated to dull redness, it readily assumes the ordinary golden colour. The Japanese use this gold, released from gold-copper alloys, in a remarkable way, for they produce, by the aid of certain pickling solutions, a beautiful patina on copper which contains only 2 per cent. of gold, while even a trace of the latter metal is sufficient to alter the tint of the patina.

With regard to theoretical views as to molecular change in metals, special care was given to a description of the work of Prof. W. Spring, of Liége, who had furnished much evidence in support of the view that polymerization of metals—that is, the rearrangement of atoms in their molecules—could take place even in *solid* alloys of lead and tin.

With reference to the passage of metals into allotropic states under slight external influences, it was stated that Debray (*Comptes rendus*, vol. xc p. 1195, 1880) has given a case of an alloy in which a simple elevation of temperature induces allotropic change in the constituent metals. It is prepared as follows: 95 parts of zinc are alloyed by fusion with 5 parts of rhodium, and the alloy is treated with hydrochloric acid, which dissolves away the bulk of the zinc, leaving a rich rhodium-zinc alloy, containing about 80 per cent. of rhodium. When this alloy is heated *in vacuo* to a temperature of 400° C., a slight explosion takes place, but no gas is evolved, and the alloy is then insoluble in *aqua regia*, which dissolved it readily before the elevation of temperature caused it to change its state. We are thus presented (as the experiment shown to the audience proved) with another undoubted case of isomerism in alloys, the unstable, soluble modification of the alloy being capable of passing into the insoluble form by a comparatively slight elevation of temperature.

The industrial importance of the passage of metals and alloys into allotropic states, and the possibility of changing the mechanical properties of metals by apparently slight influences, were fully dealt with; and the lecture concluded with a detailed description of Prof. Austen's own experiments, which have since been printed in the Philosophical Transactions of the Royal Society, the results showing that very small amounts of metallic impurities exert an extraordinary effect on the tenacity and extensibility of gold, and that small as the amounts of these impurities are, their influence is rigidly controlled by the periodic law of Newlands and Mendelejeff, the deleterious action of a metallic impurity being in direct relation to its atomic volume. The audience was asked "to remember that the knowledge of the kind of facts which had been considered comes to us from very early times, for the influence produced on metals by small quantities of added matter had a remarkable effect on the development of chemistry, mainly by sustaining the belief of the early chemists in the possibility of ennobling a base metal so as to transmute it into gold. This was the object to which they devoted life and health, and laboured with fast and vigil. We inherit the results of their labours, and their prayers have been answered in a way they little anticipated, for, from an industrial point of view, if not from a scientific one, metals are 'transmuted' by traces of impurity. Possibly we are nearing an explanation of the causes which are at work, but the fact remains that iron may be changed from a plastic material, which in ornament can be fashioned into the most dainty lines of flow, into one of great endurance, to which, for the present at least, the defence of the country may be trusted, apparently because armour-plates and missiles owe their respective qualities to the fact that carbon, manganese, and chromium have small atomic volumes."

THE LEONID METEOR-SHOWER, 1888.

AT Bristol rain fell heavily between midday on November 12 and the same time on November 13, a 5-inch gauge registering an inch and eight-tenths, which is by far the greatest downpour of the year within twenty-four hours. In the afternoon of November 13 the clouds broke, and the weather showed a disposition to become more favourable. At night the sky was moderately clear

at times. I began to watch for the return of the Leonids at 15h. 30m., but there were many clouds until about 16h., when the firmament cleared and remained nearly cloudless until 18h. During the $2\frac{1}{2}$ hours I counted 29 shooting-stars, and of these 17 were Leonids; so that the shower was pretty conspicuous. The radiant-point was at 149° + 22°, which is almost identical with the place assigned to it in previous years.

The display was noteworthy for the brilliancy of the individual meteors. I observed a large Leonid fireball at 16h. 56m., and I also saw two Leonids equal to Jupiter and one first magnitude at other times. Their paths were as follow:—

	h. m.			°	°		°	°
Nov. 13 ...	16 41 ...	♃	...	166	+ 34	to	172	+ $37\frac{1}{4}$
	16 56 ...	Fireball	...	$125\frac{1}{2}$	− 22	to	121	− $30\frac{1}{2}$
	17 19 ...	♃	...	252	+ $33\frac{1}{2}$	to	$257\frac{1}{2}$	+ 31
	17 46 ...	1	...	175	+ $66\frac{1}{2}$	to	210	+ $75\frac{1}{2}$

All these left bright streaks enduring for several seconds. The fireball at 16h. 56m. was situated very low in the southern sky, and I should not have seen it but for its brilliant flashes, which lit up the whole sky with unusual vividness. It must have been a splendid object in the English Channel and at stations in the north-west parts of France. The fine meteor at 17h. 19m. appears to have been seen also by Mr. Backhouse, at Sunderland. At Bristol it was observed near ϵ Herculis, and very close on the north-east horizon; at Sunderland it was projected near Arcturus, about 15° above the east-by-north horizon, and the most remarkable feature of its display at Sunderland was the durableness of the streak, which continued visible to the naked eye *nine minutes* after the flash which accompanied the outburst of the meteor. A comparison of the observations shows that this bolide at its disappearance was about 37 miles in vertical height above point in the North Sea near lat. $55\frac{1}{4}$° N., long. 3° E.

The recent shower appears to have been decidedly brighter than any other return of the same stream since 1879, when there occurred a very similar display to that which has just been witnessed. There was a large proportion of fine meteors from the Leonid radiant on November 13 of that year, and this seems a characteristic element of the shower.

Mr. Backhouse obtained observations at Sunderland which amply confirm those at Bristol during the apparition of 1888. He says the Leonids were "unusually numerous" on November 13. Between 16h. 48m. and 18h. 14m. he noticed about 14 of them, including one fireball, one equal to Jupiter, two equal to Sirius, and one equal to a first magnitude star. The streaks of the larger meteors belonging to this system are very pronounced in their aspect, and they sometimes brighten up considerably a few seconds after the nuclei have died away.

It is probable that this well-known group will become increasingly active every year until its maximum in 1899.

Bristol, November 17. W. F. DENNING.

NOTES.

WE understand that Prof. Ray Lankester is a candidate for the post of Deputy-Professor of Human and Comparative Anatomy in the University of Oxford, which has just been created in consequence of the continued illness of Prof. Moseley.

PROF. GIARD's lectures at the Sorbonne begin to-day. He is to deal with the general proofs of the doctrine of evolution.

IT is expected that the meeting of the International Congress of Anthropology and Prehistoric Archæology, at Paris, in 1889, will be a great success. The Congress was founded at Spezzia in 1865, and held its first session at Neuchâtel in 1866. Many members have already announced their intention of being present at the Paris meeting.

AT the ordinary meeting of the Institution of Civil Engineers, on Tuesday, November 13, the President, Sir George B. Bruce, announced that Lord Armstrong, C.B., Past-President, in the belief that the Benevolent Fund attached to the Institution was not adequately supported by the general body of members, and that it needed support, had most generously offered to contribute any sum not exceeding £5000, provided the members among them would raise a similar amount.

AT a recent meeting of the Edinburgh International Exhibition Association, the joint Committee appointed by the Association and the executive to consider the best way of disposing of the Exhibition surplus submitted its report. The Committee, among various other proposals, recommended that £1000 should be given to the Scottish Meteorological Society in aid of the Ben Nevis Observatory, and £100 to the Royal Scottish Geographical Society.

A RESOLUTE effort is about to be made to secure for North London (comprising the boroughs of Finsbury, Hackney, Islington, and St. Pancras) four great technical institutes. The Central Executive Committee has issued a circular, pointing out the need for these institutions, and appealing for funds. It is supposed that if £200,000 can be raised by private subscriptions a like sum will be contributed by the Charity Commissioners.

THE next course of Christmas lectures adapted to a juvenile auditory will be given, at the Royal Institution, by Prof. Dewar, F.R.S., the subject being "Clouds and Cloudland." They will begin on December 27. During the recess the staircases leading from the gallery of the theatre have been considerably altered in order to facilitate more speedy egress.

WE have already called attention to the fact that an effort is being made to secure for Bedford College (for ladies), York Place, Baker Street, London, new chemical and physical laboratories. The College has been among the most successful of the institutions which send up graduates to the science degrees at the University of London; but its students have hitherto been severely handicapped by the inadequacy of the laboratory accommodation. The opportunity has occurred of securing fresh building-ground adjoining the College, and plans have been prepared for the proposed additional laboratories. The offer must be definitely made before Christmas, and the estimated expenditure will be at least £3000. The Council do not feel warranted in going forward unless they have at least a considerable portion of this sum promised, for which they are entirely dependent on voluntary contributions. Subscribers' names will be received by the lady-resident at the College. An application for assistance is also being made to the City Companies. Bedford College is the only institution exclusively for ladies which provides first-class practical instruction of this kind.

THE work of the United States Fish Commission during the past season seems to have been even more than usually successful. Stations capable of hatching four hundred million codfish-eggs have been established at Gloucester and Wood's Holl; and, according to *Science*, these stations will probably put at least one-fourth of that number of cod-fry into the Atlantic Ocean during the present season. It is expected, therefore, that codfish will soon be more plentiful on the coast of New England than they were years ago, and that a lost industry worth millions of dollars will be restored to that part of the United States.

THE Committee on Science and the Arts, of the Franklin Institute, of Pennsylvania, wishes to make known the fact that it is empowered to award, or to recommend the award of, certain medals for meritorious discoveries and inventions which tend to the progress of the arts and manufactures. These medals are:—(1) The Elliott Cresson Medal (gold), founded by the legacy

of Elliott Cresson, of Philadelphia, and conveyed to the Trustees of the Franklin Institute. By the Act of the Institution, May 17, 1849, the Committee on Science and the Arts was designated and empowered to award this medal, and the Committee decided to grant it, after proper investigation and report by a sub-committee, either for some discovery in the arts and sciences, or for the invention or improvement of some useful machine, or for some new process, or combination of materials in manufactures, or for ingenuity, skill, or perfection in workmanship. (2) The John Scott Legacy Premium and Medal (twenty dollars and a medal of copper), founded in 1816, by John Scott, a merchant of Edinburgh, Scotland, who bequeathed to the city of Philadelphia a considerable sum of money, the interest of which should be devoted to rewarding ingenious men and women who make useful inventions. Upon request made to the Secretary of the Franklin Institute, full information will be sent respecting the manner of making application for the investigation of inventions and discoveries; furthermore, the Committee on Science and the Arts will receive and give respectful consideration to reports upon discoveries and inventions which may be sent to it with the view of receiving one or the other of the above-named awards. Full directions as to the manner and form in which such communications should be made, will be sent on application.

THE late Prof. Edward Tuckerman made a choice collection of books and papers relating to lichens, some four hundred numbers in all. These works have been presented by Mrs. Tuckerman, in accordance with her husband's wish, to Amherst College Library; and it is proposed to keep the collection by itself under the name of the "Tuckerman Memorial Library," and to render it worthy of the name by making it as complete as possible in its own department. Some persons interested in this specialty may like to assist in maintaining and completing the collection, on the understanding that it shall always be available to public use. Mr. Wm. I. Fletcher, the Librarian of Amherst College, has therefore issued a notice to the effect that he will be glad to receive contributions, either in money or in material (especially rare monographs that may have escaped Prof. Tuckerman's notice), to this memorial to an eminent scholar and man of science. Whatever money may be contributed will be kept as a fund of which only the income will be employed in making additions to the collection, or in repairs and rebinding. The sum of 1000 dollars would probably suffice as such a fund.

AT the meeting of the Scientific Committee of the Royal Horticultural Society on November 13, Mr. Henslow showed specimens of several species of plants which are propagated by cleistogamous flower-buds. By that means, while retaining a dwarf habit, they are able to multiply very rapidly, and to extend over considerable areas in a tennis-lawn. Although none of them are perennials, they remain so reduced in size that they are not exterminated by the mowing-machine periodically cutting them down. The result is that each species has more or less completely covered certain patches of ground, to the almost entire exclusion of everything else. The plants in question are *Cerastium glomeratum*, *Montia fontana*, *Trifolium procumbens*, *Sagina procumbens*, *Alchemilla arvensis*, *Veronica arvensis*, and *Poa annua*. Mr. Henslow added that he had observed, many years ago, *Trifolium subterranean* flourishing in the same way on the close-cut grass in Kew Gardens, on the site of the present rockery.

EARTHQUAKES occurred at Vyernyi on October 30, at 6 a.m.; at Digne, in the Department of the Lower Alps, on November 1, at 1.36 a.m.; in Sikkim on the 9th inst.; and in various places in California on the 19th inst.

ON the night of Saturday, November 3, about 8 o'clock, he tens of thousands of sheep folded in the large sheep-breeding districts north, east, and west of Reading were taken with a sudden panic, jumping their hurdles, escaping from the fields, and running hither and thither. Early on Sunday morning the shepherds found their animals under hedges and in the roads, panting as if they had been terror-stricken. Messrs. Oakshott and Millard, who write to us on the subject, suggest that a slight earthquake may have been the cause of this strange incident.

THE third general meeting of the Italian Meteorological Society was held at Venice, from September 14 to 21, in the Institute of Music of Benedetto Marcello. Reports of the various papers read are being published in the *Bollettino Mensuale* of the Society. There were discussions on various questions connected with general meteorology and climatology; meteorology applied to hygiene, and hydrology; and geodynamics.

A SUMMARY of the results of two interesting papers read by General Strachey at the recent meeting of the British Association on the meteorology of the Red Sea and Cape Guardafui, prepared from data collected by the Meteorological Office, is contained in the Proceedings of the Royal Geographical Society for November. In 1882 the Admiralty called attention to the precautions necessary in rounding Cape Guardafui from the southward, wrecks having frequently occurred in foggy weather and at night-time, owing to vessels being out of their reckoning from the great variation in the drift of the currents. The paper shows that, although not always infallible, the change of sea temperature in approaching the land is of great assistance in determining the position when near this dangerous coast. The late Captain Symington called attention to this important fact in 1880.

THE Meteorological Office of the Argentine Republic, which was established at Cordoba by Dr. Gould fourteen years ago, and to which we are chiefly indebted for our knowledge of the climate of this vast region, continues to follow the same useful lines under its present Director, Mr. G. G. Davis. Volume vi. of the *Anales*, recently published, contains exhaustive discussions on the climate of five places, from as long a series of observations as was available in each case. The observers are supplied with verified instruments, and the stations are regularly inspected. Several new stations have been established in the more remote districts.

MOST ornithologists have hitherto been of opinion that birds during their yearly migrations never cross the Caucasus chain of mountains, and that on their way towards the north, and on their return, they take the route *viâ* the shores of the Black Sea or of the Caspian. It appears, however, from recent observations made by M. Rossikoff, and communicated to the *Caucasus* newspaper, that such is not the case. On October 9, while on the banks of the Aragva, close by the Ananur station, he saw a number of large birds on the summit of the Tsikhes-dziri Mountain, and they proved to be a flock of some 100 to 150 white storks (*Ciconia alba*), which were crossing the Caucasus chain on their return journey. It appeared also from information gathered from the inhabitants that cranes, herns, and ducks have been often seen in that part of the main chain on their migrations.

TWO new crystalline compounds of arsenious oxide with sodium bromide and sodium iodide have been prepared by Prof. Rüdorff, of Charlottenburg. They form two additional members of an interesting isomorphous group discovered two years ago by Prof. Rüdorff, having the general formula $MX + 2As_2O_3$, where M may be potassium, sodium, or ammonium, and X represents chlorine, bromine, or iodine. In his former memoir, published in 1886, the following members of the series are described, and accurate analyses given:—$KI + 2As_2O_3$, $KBr + 2As_2O_3$, $KCl + 2As_2O_3$, $NH_4I + 2As_2O_3$, and $NH_4Br + 2As_2O_3$. These five salts are beautifully isomorphous,

forming hexagonal prisms with rhombohedral terminations, almost undistinguishable from each other on account of their extreme similarity. The two new compounds just announced in the current *Berichte* are the sodium compounds $NaBr + 2As_2O_3$ and $NaI + 2As_2O_3$. The former was obtained by dissolving 20 grammes arsenious oxide and 120 grammes sodium bromide in 350 c.c. boiling water, filtering the somewhat turbid liquid thus obtained, and allowing the clear solution to cool very slowly. On suspending in it a plate of cold glass, crystals of the compound were immediately deposited upon it, which proved on microscopic examination to consist of the hexagonal prisms common to the whole series. The crystals were then washed upon the large glass plate with cold water, dried superficially between filter-paper, afterwards at 130°, and submitted to analysis. The numbers obtained indicate the formula given above. The crystals, as in the case of many other double compounds, are decomposed into their constituents by contact with warm water. The second compound, $NaI + 2As_2O_3$, was similarly obtained, using 60 grammes sodium iodide, 22 grammes arsenious oxide, and dissolving them in half a litre of hot water. On allowing a glass plate to stand twenty-four hours in the solution, it was found to be covered with six-sided prisms, apparently identical in shape with those of the other compounds of the series previously obtained. It is interesting that both these compounds can be readily obtained by the addition of a sufficient quantity of bromide or iodide of sodium to a solution of sodium arsenite. The corresponding compound with chloride of sodium has not yet been prepared. Thus another series of analogous salts is added to the list of isomorphous groups, the number of which is steadily increasing. These groups are of especial interest to the crystallographer, inasmuch as they appear likely to be of invaluable assistance in our attempts to grasp the fundamental problem, "Why does a definite chemical compound crystallize in any particular system?"

THE County of Middlesex Natural History and Science Society has issued its Transactions during the session 1887-88. The volume includes, besides an abstract of an introductory address by Dr. Archibald Geikie, papers on subjects of scientific interest by Prof. Flower, Mr. W. Lant Carpenter, Dr. Sydney T. Klein, and others.

THE new number of the Journal of the Anthropological Institute (vol. xviii. No. 2) is one of great interest. Mr. Galton offers some suggestive remarks on replies by teachers to questions respecting mental fatigue, and Mr. J. Venn has a careful paper on "Cambridge Anthropometry." In a paper on the races of the Babylonian Empire, Mr. G. Bertin sets forth some conclusions which are so different from, and even opposed to, "accepted notions," that he hesitated a long time, he says, "before surrendering himself to the evidence."

DR. H. TEN KATE, in a letter to *Science*, expresses dissent from the opinions lately set forth by Dr. Daniel G. Brinton, as to "the alleged Mongolian affinities of the American race." Dr. Brinton holds that there are no such affinities, whereas to his opponent it seems obvious that the native race of America cannot be properly understood without reference to the Mongolians. Summing up his views on the subject, Dr. H. Ten Kate says:—"I wish to say that Dr. Brinton's argumentation against the affinity between Americans and Mongolians is based upon entirely wrong reasoning. If the reasons he gives were correct, then the classification of the other races of the human species would be equally wrong; for in each of them peoples are grouped together, which, although related by physical characteristics, are linguistically and ethnologically entirely different from each other, not to speak of the difference in their psychological and social evolution. When I admit that the native Americans are Mongoloids, I do not necessarily imply that America has been populated from Asia or elsewhere. However, if we accept the theory of evolution, this is the most probable explanation of the observed facts. But, leaving the doubtful origin of the Americans, and of their languages and arts, out of the question, I maintain that there is a physical similarity, racial affinity, and relationship between the indigenous Americans and the Mongolians in the widest sense. This is, in the present state of anthropological knowledge, an undeniable fact. He who denies it does not believe in physical anthropology; and not to recognize this branch of science is equal to denying natural history in general."

IN the last Administration Report of the Island of Dominica, which has just been laid before Parliament, the President estimates the original Carib population at about three hundred, or something under that figure. The children are healthy, and the numbers of Caribs, according to their own accounts, are not decreasing, but they are gradually becoming so intermixed with the Negroes that the pure Carib, "Franc Carib," will soon be non-existent. The Carib quarter, a reserve of large extent on the Windward side of Dominica, is wholly occupied by them. They are very peaceable and retiring; they live on vegetables and fruits, which they cultivate, and on fish. They keep their roads in good order. Their children appear to be very intelligent, but there is no school in the Carib quarter, though the people are very anxious to have one.

A SOCIETY has been formed in France, under the patronage of many men of high scientific and political standing, for the purpose of developing a proper system of physical education. The leader of the new movement is M. Philippe Daryl, *alias* Paschal Grousset, who, when a very young man, took an active part in the Paris insurrection of 1871. He afterwards lived in England, whence he sent interesting letters to the *Temps*.

THE *Madras Mail* says that Dr. Thurston, the Superintendent of the Madras Museum, has been requested by the Board of Revenue to visit various electrical establishments in Europe in order to select an electric globe light to shine in twenty fathoms of water. Such a light has long been wanted at the pearl fisheries, for, up to the present, the work of the fisheries has been confined to banks situated at a less depth than twenty fathoms.

THE next *conversazione* of the Royal Microscopical Society will be held on Wednesday, the 28th instant, at 8 o'clock.

THE additions to the Zoological Society's Gardens during the past week include a —— Monkey (*Cercopithecus* sp. inc.) from South Africa, presented by Mr. L. B. Lewis; an Indian Antelope (*Antilope cervicapra* ♂) from India, presented by Mr. J. W. Shand-Harvey; a Himalayan Bear (*Ursus tibetanus* ♀) from Northern India, presented by the Regiment of the 2nd Life Guards; an Axis Deer (*Cervus axis*) from India, presented by Lady Donaldson; a Roseate Cockatoo (*Cacatua roseicapilla*) from Australia, presented by Mrs. Wood; two American Bitterns (*Botaurus lentiginosus*) from North America, presented by Mr. J. B. Williams; a Peregrine Falcon (*Falco peregrinus*), captured at sea, presented by Mr. R. H. Armstrong; a Spotted Salamander (*Salamander maculosa*), European, presented by Mr. F. E. B. Roper, F.Z.S.; two Swans (*Cygnus olor* ♀ ♀), European, a Royal Python (*Python regius*) from West Africa, deposited; two Golden Plovers (*Charadrius pluviatilis*), British, purchased.

OUR ASTRONOMICAL COLUMN.

THE BRAZILIAN TRANSIT OF VENUS EXPEDITIONS, 1882.—We have received vol. iii. of the "Annaes do Imperial Observatorio do Rio de Janeiro," containing the reports of the

Brazilian expeditions to observe the transit of Venus of December 1882. The volume, which is a handsome quarto of some 750 pages, and is fully illustrated with photographs of the various stations and instruments, contains reports from three stations, the weather at the Imperial Observatory at Rio de Janeiro itself, which should have been a fourth station, having proved cloudy and wet. It had been at first intended to send an expedition to Cuba, but as the German astronomers had occupied a position there, the little Island of St. Thomas, belonging to Denmark, was chosen instead. St. Thomas paired well with the southern station, Punta Arenas, in Patagonia, for the duration was much shortened at the former place and slightly lengthened at the latter, the sun being high at both stations, and ingress and egress at both taking place nearly symmetrically with regard to the meridian. The entire transit was also seen from the remaining station, Olinda, near Pernambuco, where ingress was somewhat retarded and egress much accelerated. The observations were all made by the method of projection, in order that the disturbing effects of irradiation might be got rid of as far as possible. The St. Thomas expedition which was under the command of Baron de Teffé, possessed three equatorials, and Dr. H. Draper promised to supply a photoheliograph, but his lamented death prevented the carrying out of his generous intention. The Olinda expedition, commanded by M. J. de Oliveira Lacaille, had two equatorials; whilst M. Cruls, the chief of the Punta Arenas party, had but one; the largest telescope in each case being 6¼ inches in aperture. M. Cruls selected a site for his party within a mile of that occupied by Dr. Auwers with the German expedition; for the Brazilian Parliament having at the last moment refused the necessary credit for the expedition, and the required funds having been only provided by the personal generosity of the Emperor, the expedition did not arrive at the place until late, and it seemed better to take advantage of the German choice of position rather than lose time by surveying for a fresh site at a distance. The observations at each of these three stations were successful, the resulting parallax from the internal contacts being 8″·808. A large part of the volume is devoted to a report of the voyage of the corvette *Parnahyba*, by Captain L. Saldanha de Gama, the captain who conveyed the southern observing party to their station, and to a description of the natural history of Tierra del Fuego.

THE TAIL OF COMET 1887 *a* (THOME).—Prof. Bredichin has discussed in the *Bulletin* of the Imperial Society of Naturalists of Moscow, 1888, Nos. 2 and 3, the observations of the direction of the tail of this comet. The comet was discovered by Mr. Thome, of Cordoba, on January 18, 1887, and it was remarkable for the smallness of its perihelion distance, the complete absence of any nuclear condensation in the head, and the length, straightness, and narrowness of the tail. Prof. Bredichin finds that the tail manifestly belongs to his third type, viz. those in which the repulsive force, $1 - \mu$, does not exceed 0·1. He suggests, as the rate of outflow in comets of short perihelion distance is much more rapid at perihelion passage than later, and as the comet was not discovered until a week after perihelion, that the lighter materials may already have been driven off and reduced to such a degree of tenuity as to be invisible, leaving only substances of heavy atomic weight. As is well known, he associates his first type of tail, that in which the repulsive force is greatest, with hydrogen, the more ordinary second type with the hydrocarbons; and he suggests in the case of the present comet that elements with atomic weights like those of gold, mercury, and lead, would furnish a tail of the character observed. Some comets, however, which do not approach the sun closely, have tails only of the third type. If, then, Prof. Bredichin's explanation is to be received in its entirety, hydrogen and hydrocarbons are not always constituents of cometary tails.

ASTRONOMICAL PHENOMENA FOR THE WEEK 1888 NOVEMBER 25—DECEMBER 1.

(FOR the reckoning of time the civil day, commencing at Greenwich mean midnight, counting the hours on to 24, is here employed.)

At Greenwich on November 25

Sun rises, 7h. 38m.; souths, 11h. 47m. 19·0s.; sets, 15h. 57m.: right asc. on meridian, 16h. 6·7m.; decl. 20° 54′ S. Sidereal Time at Sunset, 20h. 17m.

Moon (at Last Quarter November 26, 17h.) rises, 21h. 1m.*; souths 4h. 56m.; sets, 12h. 40m.: right asc. on meridian, 9h. 14·7m.; decl. 18° 7′ N.

Planet.	Rises.		Souths.		Sets.		Right asc. and declination on meridian.	
	h. m.		h. m.		h. m.		h. m.	° ′
Mercury..	5 53	...	10 38	...	15 23	...	14 57·0 ...	14 59 S.
Venus.....	10 37	...	14 18	...	17 59	...	18 37·4 ...	25 2 S.
Mars......	11 30	...	15 29	...	19 28	...	19 49·1 ...	22 32 S.
Jupiter....	8 32	...	12 33	...	16 34	...	16 52·1 ...	22 6 S.
Saturn....	21 48*	...	5 14	...	12 40	...	9 32·2 ...	15 37 N.
Uranus...	3 31	...	8 57	...	14 23	...	13 16·0 ...	7 23 S.
Neptune..	15 50	...	23 35	...	7 20*	...	3 56·2 ...	18 39 N.

* Indicates that the rising is that of the preceding evening and the setting that of the following morning.

Occultations of Stars by the Moon (visible at Greenwich).

Nov.	Star.	Mag.	Disap.	Reap.	Corresponding angles from vertex to right for inverted image.
			h. m.	h. m.	° °
26 ...	37 Leonis	... 6 ...	7 25	near approach	178 —
28 ...	B.A.C. 3996	... 6 ...	1 44	near approach	301 —
30 ...	B.A.C. 4572	... 6 ...	5 59	near approach	124 —

Nov.	h.	
25 ...	13 ...	Saturn in conjunction with and 1° 31′ south of the Moon.
Dec.		
1 ...	11 ...	Saturn stationary.

Variable Stars.

Star.	R.A.	Decl.			
	h. m.	° ′		h. m.	
U Cephei	0 52·4 ...	81 16 N. ...	Nov. 25,	0 27	*m*
			,, 30,	0 6	*m*
S Arietis	2 9·8 ...	24 32 N. ...	Dec. 1,		*m*
λ Tauri...	3 54·5 ...	12 10 N. ...	Nov. 26,	20 44	*m*
			,, 30,	19 36	*m*
ζ Geminorum ...	6 57·5 ...	20 44 N. ...	,, 29,	2 0	*m*
R Canis Majoris...	7 14·5 ...	16 12 N. ...	,, 25,	0 41	*m*
			,, 26,	3 57	*m*
U Monocerotis ...	7 25·5 ...	9 33 S. ...	,, 27,		*m*
U Hydræ	10 32·0 ...	12 48 S. ...	,, 26,		*m*
S Leonis	11 5·1 ...	6 4 N. ...	,, 30,		*M*
R Scuti	18 41·5 ...	5 50 S. ...	,, 29,		*M*
η Aquilæ	19 46·8 ...	0 43 N. ...	,, 29,	18 0	*m*
T Aquarii	20 44·0 ...	5 34 S. ...	,, 27,		*m*
T Vulpeculæ ...	20 46·7 ...	27 50 N. ...	,, 29,	21 0	*M*
			,, 3[illegible],	22 0	*m*
Y Cygni	20 47·6 ...	34 14 N. ...	,, 25,	2 18	*m*
			,, 28,	2 12	*m*
δ Cephei	22 25·0 ...	57 51 N. ...	,, 25,	4 0	*M*
			,, 28,	21 0	*m*

M signifies maximum; *m* minimum.

Meteor-Showers.

The most interesting periodical shower of the week is that of the *Andromedes*, the stream connected with Biela's comet, but no remarkable display can be expected from it this year: max. Nov. 27; radiant about R.A. 25°, Decl. 44° N. Other showers of the week are as follow:—

	R.A.	Decl.	
Near λ Persei	60° ...	50° N. ...	Very swift.
,, β Canum Venaticorum	190 ...	42 N. ...	Swift; streaks.

GEOGRAPHICAL NOTES.

Two letters relating to Dr. Nansen's expedition across Greenland have been published—one from Dr. Nansen himself to Mr. Augustin Gamel, Copenhagen, who is defraying the expenses of the expedition; the other from Mr. Sverdrup, one of Dr. Nansen's companions, to his father. The letters were sent forward from Godthaab by two *kajak*-men, who delivered them to the captain of the *Fox*, at Ivigtut. The following is a translation of Dr. Nansen's letter:—

GODTHAAB, *October* 4.

I have at last the great joy to report to you that Greenland has been successfully crossed from east to west. I regret that the very short time left to me before despatching my messengers will not permit any detailed account. I can just jot down a few words to be forwarded by the *kajak*-men. I am sending southwards in the hope of stopping the *Fox* at Ivigtut, and getting her to wait for us and take us home this autumn. But

in case the *kajak*-messenger should catch the steamer without inducing her to wait for us, I write these few lines just to inform you that we are all alive and well.

As you will know, we left the *Jason*, the Norwegian sealer, on July 17, and expected to reach the shore the next day. But in this we were sadly disappointed. Screwing ice, maëlstroms, impassable ice, where it was alike impossible to row or to drag the two boats, stopped us. One of the boats was stove in, but we got it repaired again. We drifted seawards at a speed of thirty sea miles in the twenty-four hours. Drifted in the ice for twelve days. Strove hard to get to the shore, were three times on the point of succeeding, but were as often carried out to sea again by a current stronger than our power of rowing. Were once, for a whole day and night, very near perishing in tremendous breakers of the sea against the ice-rim. After twelve days' drifting about, we managed at last to get ashore near Andretok, north of Cape Farewell, at 61° and some minutes of northern latitude. We rowed again northwards, reaching Uminik, from which point the crossing of the inland ice began on August 15.

We directed our course for Christianshaab on the western coast. Encountered severe snowstorms and had heavy ground. Estimating that it would be too late to reach Christianshaab in time for this autumn's vessel, we altered our course and steered for Godthaab, the ice-fields in that direction having, besides, been hitherto trodden by no one. After altering course, reached height of 10,000 feet, with temperature of 40° to 50° C. below zero. For several weeks we remained at an altitude of over 9000 feet. Tremendous storms, loose, new-fallen snow, enormously difficult passage. Towards end of September we reached at last the western side above Godthaab. Had a perilous descent, on ugly and very uneven ice, but got safely down to Ameralik Fjord. Managed to build a kind of boat from floor of tent, bags, bamboo reeds, and willow branches. In that frail craft Sverdrup and I rowed away, and arrived here on October 3. The four men are left at Ameralik, living there on short rations fare, but will be sent for as soon as possible.

There you have in short outline our Saga. We are all perfectly well, and everything has been in the best order. I hope that we may catch this steamer, and that instead of this letter you may see our sunburnt faces.

With many greetings, yours ever devotedly,

FRITHIOF NANSEN.

P.S.—Just now the *kajak*-men must leave, profiting by the favourable weather. They have 300 miles to make before getting to Ivigtut.

The following is a translation of Mr. Sverdrup's letter:—

GODTHAAB, *October* 4, 1888.

We arrived safely here yesterday after forty-six days' wandering from east to west. It did not prove so easy to get on shore from the *Jason* as we had expected. We got into formidable ice-screwings, and the current took us southwards and out from the shore, so that we had twelve days' very hard work before getting to land, and that 300 miles more to the south than we had intended. We began at once to work back along the coast, and this took us another twelve days, so that we did not begin our crossing of the ice before August 15. The ascent was very fortunate, as we chanced to find comparatively easy ice to climb up. We shaped our course for Christianshaab, but after getting up to 7500 feet we were attacked by a northern snowstorm. We resolved, therefore, to set our course for Godthaab, the distance being shorter, and there being a better chance of favourable winds. I may truly say that we had a hard time of it. The snow and ice were very heavy, and the weather was trying. For nearly three weeks we were up at nearly 10,000 feet, and had a temperature of −40° to −50° C. Only for four days were we snowbound. After all, we have to be thankful it was not worse. After getting down from the inland ice on the western coast, we had before us some ninety miles of barren country, of which the half lay along a fjord. We tried to cross here, but found it too hard work; so we managed to construct a kind of boat from the bottom of the tent and some bags, and in that, after four days' rowing, Dr. Nansen and I reached here, where we had the most cordial reception from all the inhabitants of the colony. Two boats have now been sent to the bottom of the fjord to fetch our comrades. The regular vessel has long since left, but some 250 or 300 miles further south there is supposed to be, at Ivigtut, a steamer loading for Copenhagen, and we are now sending a *kajak*-message in order to stop that steamer if possible. We have but little hope of that, however, and are preparing to pass the winter here. That may be very comfortable after all, but of course we would prefer getting home. I must hurry up, as we are now going to dine with the parson, and, in fact, we have not had time for anything, as since arriving here we have gone from one social party to another. You may see from that how well we are off. I was the only one of our whole party who got over all the tremendous fatigues without the smallest ailment. I am, and have been all the time, as fresh and sound as a fish.

DR. GEORG SCHWEINFURTH has started upon an Oriental journey. He is going to Arabia first, to continue his studies of the coffee-plant.

THE FOUNDATION-STONES OF THE EARTH'S CRUST.[1]

DO we know anything about the earth in the beginning of its history—anything of those rock masses on which, as on foundation-stones, the great superstructure of the fossiliferous strata must rest? Palæontologists by their patient industry have deciphered many of the inscriptions, blurred and battered though they be, in which the story of life is engraved on the great stone book of Nature. Of its beginnings, indeed, we cannot yet speak. The first lines of the record are at present wanting—perhaps never will be recovered. But apart from this—before the grass, and herb, and tree, before the "moving creature in the water," before the "beast of the earth after his kind,"—there was a land and there was a sea. Do we know anything of that globe, as yet void of life? Will the rocks themselves give us any aid in interpreting the cryptogram which shrouds its history, or must we reply that there is neither voice nor language, and thus accept with blind submission, or spurn with no less blind incredulity, the conclusions of the physicist and the chemist?

The secret of the earth's hot youth has doubtless been well kept. So well that we have often been tempted to guess idly rather than to labour patiently. Nevertheless we are beginning, as I believe, to feel firm ground after long walking through a region of quicksands; we are laying hold of principles of interpretation, the relative value of which we cannot in all cases as yet fully apprehend—principles which occasionally even appear to be in conflict, but which will some day lead us to the truth.

I shall not attempt to give you an historical summary, but only to lay before you certain facts for which I can answer, and to indicate the inductions which these, as it seems to me, warrant. If I say little of the work of others, it is not from a desire of taking credit to myself, but because it is immaterial for my present purpose who first made a particular observation and how far his inductions therefrom were correct. The acknowledgment of good work would involve repudiation of bad, and for that, so far as persons are concerned, it seems hardly fair to use the present occasion. So, in the outset of this lecture, I will once for all make a statement which I have sometimes thought of invariably using, like a prefatory invocation, "You are free to suppose that everything herein has been said by somebody, somewhere," but I will add that, as far as possible, every assertion has been personally verified.

The name Cambrian has been given to the oldest rocks in which fossils have been found. This group forms the first chapter in the first volume, called Palæozoic, of the history of living creatures. Any older rocks are provisionally termed Archæan. These—I speak at present of those indubitably underlying the Cambrian—exhibit marked differences one from another. Some are certainly the detritus of other, and often of older, materials—slates and grits, volcanic dust and ashes, even lava-flows. Such rocks differ but little from the basement-beds of the Cambrian; probably they are not much older, comparatively speaking. But in some places we find, in a like position, rocks as to the origin of which it is more difficult to decide. Often in their general aspect they resemble sedimentary deposits, but they seldom retain any distinct indications of their original fragmental constituents. They have been metamor-

[1] An evening discourse, delivered at the Bath meeting of the British Association by Prof. T. G. Bonney, D.Sc., LL.D., F.R.S., &c

phosed, the old structures have been obliterated, new minerals have been developed, and these exhibit that peculiar orientation, that rudely parallel arrangement, which is called foliation. Except for this some masses are fairly homogeneous, while some exhibit a distinct mineral banding which is usually parallel with the other structure. These rocks are the gneisses and schists—the latter term, often vaguely used, I always restrict to rocks which exhibit a true foliation. In some schists the mineral constituents are comparatively minute, in others they are of considerable size. In the former case we may often venture to affirm that the rock is a metamorphosed sediment; in the latter its original condition is a matter of conjecture. Rocks of the former class often appear, to use no stronger word, to lie above, and so to be less ancient than those of the latter, and beneath that comes a coarser and more massive series, in which granitoid rocks are common. In these last foliation is often inconspicuous, and the rocks in consequence are not markedly fissile.

That these rocks are older than the Cambrian can often be demonstrated. Sometimes it can even be proved that their present distinctive character had been assumed before the overlying Cambrian rocks were deposited. Such rocks, then, we may confidently bring forward as types of the earth's foundation-stones. As the inscriptions buried in the Euphrates Valley tell us the tongue of Accad in the days prior to the coming of the Semite, so these declare what then constituted the earth's crust. If in such rocks we find any peculiarities of mineral composition or structure, these may legitimately be regarded as distinctive. We have only to beware of mistaking for original those which are secondary and subsequently impressed.

In other parts of the world we find rocks of like characters with those above named, the age of which cannot be so precisely fixed, though we can prove them to be totally disconnected from and much older than the earliest overlying stratum. To assert that these rocks are contemporary with the others is obviously an hypothesis which rests on the assumption that community of structure has some relation to similarity of origin. I am well aware that attempts have been made to discredit this. But if we eliminate difficulties which are merely sophistical—those, I mean, created by the use of ambiguous or misleading terms—if we acknowledge those due to our limited means of investigation, such as that of distinguishing a rock crushed *in situ* from one composed of transported fragments—in other words, of separating in every case a superinduced from a primary structure, and if we allow for others due to the limitation of our instrumental and visual powers, I do not hesitate, as the result of long and, I hope, careful work, to assert that certain structures are very closely related to the past history of a rock, and that in very many instances our diagnosis of the cause from its effect is not less worthy of confidence than that of an expert in pathology or physiology. Resemblances of structures, different in origin, do, no doubt, sometimes occur—resemblances not seldom due to partial correspondence in the environments; but in regard to these it is our duty to labour patiently till we succeed in distinguishing them. The difficulty of the task does not justify us, either in abandoning it in despair, or in sitting down, after a few hasty observations, to fashion hypotheses which have no better foundation than our own incompetence or idleness.

As it is impossible in the time at my disposal to demonstrate the proposition, I must assume what I believe few, if any, competent workers will deny, that certain structures are distinctive of rocks which have solidified from a state of fusion under this or that environment; others are distinctive of sedimentary rocks; others again, whatever may be their significance, belong to rocks of the so-called metamorphic group. I shall restrict myself to indicating, by comparison with rock structures of which the history is known, what inferences may be drawn as to the history of the last-named rocks, which, as I have already stated, are in some cases examples of the earth's foundation-stones, while in others, if they are not these, they are at any rate excellent imitations.

Let us proceed tentatively. I will put the problem before you, and we will try to feel our way towards a solution. Our initial difficulty is to find examples of the oldest rocks in which the original structures are still unmodified. Commonly they are like palimpsests, where the primitive character can only be discerned, at best faintly, under the more recent inscription. Here, then, is one of the best which I possess—a Laurentian gneiss from Canada. Its structure is characteristic of the whole group; the crystals of mica or hornblende are well defined, and commonly have a more or less parallel arrangement; here and there are bands in which these minerals are more abundant than elsewhere. The quartz and the felspar are granular in form; the boundaries of these minerals are not rectilinear, but curved, wavy, or lobate; small grains of the one sometimes appear to be inclosed in larger grains of the other. Though the structure of this rock has a superficial resemblance to that of a granite of similar coarseness, it differs from it in this respect, as we can see from the next instance, a true granite, where the rectilinear outline of the felspar is conspicuous. Here, then, is one of our problems. This difference of structure is too general to be without significance. What does it mean?

It is more difficult to obtain examples of schist of like geological age, wholly free from subsequent modification. Apparently the structure and composition of the rock have rendered it more liable to disturbance. But those exhibited, though by no means perfect examples, may serve to indicate the structure of an Archæan schist, consisting mainly of quartz and mica. We may take them as representative of a considerable series of rocks, which are often associated in such a manner as to suggest that, notwithstanding their present crystalline condition, they had a sedimentary origin. Can this inference be justified?

How shall we attack this problem? Clearly, the most hopeful way is by proceeding from the known to the unknown. Now, among the agents of change familiar to geologists, three are admittedly of great importance; these are water, heat, and pressure. As probably almost all changes in nature, with which we have to deal, have occurred in the presence of water, but those due to it alone are generally superficial, I shall assume its presence, and not attempt to isolate its effects. But we must endeavour to ascertain the results of pressure and heat, when acting singly and in combination, in modifying rocks of a known character; admitting, however, that probably while the one agent has been dominant, the other has not been wholly inoperative.[1]

The first effect of pressure due to great earth-movements is to flatten somewhat the larger fragments in rocks, and to produce in those of finer grain the structure called cleavage. This, however, is a modification mainly mechanical. It consists in a re-arrangement of the constituent particles, mineral changes, so far as they occur, being quite subordinate. But in certain extreme instances the latter are also conspicuous. From the fine mud, generally the result of the disintegration of felspar, a mica, usually colourless, has been produced, which occurs in tiny flakes, often less than one-hundredth of an inch long. In this process, a certain amount of silica has been liberated, which sometimes augments pre-existing granules of quartz, sometimes consolidates independently as microcrystalline quartz. Carbonaceous and ferruginous constituents are respectively converted into particles of graphite and of iron oxide. Here is an example of a Palæozoic rock, thus modified. It originally consisted of layers of black mud and gray silt. In the former, this filmy mica has been abundantly developed; it is present also, as we might expect, to some extent in the latter. Observe that the original banded structure, notwithstanding the pressure, has not been obliterated. Another point also demands notice. The black lines in the section indicate the direction of the cleavage of the rock, which is, roughly speaking, at right angles to the pressure which has most conspicuously affected the district, while the microfoliation, as we may call it, appears to be parallel to the original bedding, and is thus anterior to the dominant cleavage. The two may form parts of a connected series of movements, but, at any rate, they are so far separated that the pressure which produced the one, acted, roughly speaking, at right angles to that which gave rise to the other, and the folia were developed before they were bent and torn.

Let us now pass on to examine the effects of pressure when it acts upon a rock already crystalline. Here, obviously it is comparatively unimportant whether the original rock was a true granite or a granitoid gneiss; for at present we are only concerned with the effect of pressure on a fairly granular crystalline rock. But in the resultant structures there are, as it seems to me, differences which are dependent upon the mode in which pressure has acted. They are divisible into two groups: one indicating the result of simple direct crushing, the other of crushing accompanied by shearing. In the former case, the rock

[1] Heat will, of course, result from the crushing of rock. This some consider an important factor in metamorphism, but I have never been able to find good evidence in favour of it, and believe that as a rule the rocks yield too slowly to produce any great elevation of temperature.

mass has been so situated that any appreciable lateral movement has been impossible; it has yielded like a block in a crushing-machine. In the latter, a differential lateral movement of the particles has been possible, and it has prevailed when (as in the case of an overthrust fault) the whole mass has not only suffered compression, but also has travelled slowly forward. Obviously, the two cases cannot be sharply divided, for the crushing up of a non-homogeneous rock may render some local shearing possible. Still it is important to separate them in our minds, and we shall find that in many cases the structure, as a whole, like the cleavage of a slate, results from a direct crush; while in others the effects of shearing predominate. The latter accordingly exhibit phenomena resembling the effects of a tensile stress. Materials of a like character assume a more or less linear arrangement, the rock becomes slightly banded, and exhibits, as has been said, a kind of fluxion structure. This phrase, if we are careful to guard ourselves against misconception, is far from inappropriate. The mass gradually assumes a fragmental condition under the pressure, and its particles as they shear and slide under the effects of thrust, behave to some extent like those of a non-uniform mass of rock in a plastic condition, as, for example, a slaggy glass. But we must be on our guard, lest we press the analogy too far. The interesting experiments which have been made on the flow of solids, and on rolled-out plastic substances, while valuable as illustrations, represent, as it seems to me, a condition of things which must be of rare occurrence in a rock mass, pulverized by mechanical forces only. If I am to reason from them, I must regard the rock not as a fragmental solid—if the phrase be permissible—but as an imperfect fluid; that is to say, I must consider them as illustrative of structures in rocks which have yet to assume—not have already assumed—a crystalline condition.

Illustrations of the effects of direct crushing in a granitoid rock are common in the Alps. Those of a shearing crush are magnificently developed near the great overthrust faults in the north-west Highlands of Scotland.

In the former case, where a granitoid rock has been affected only to a moderate extent, and the resulting rock in a hand specimen would be called a gneiss without any very definite mineral banding, we find that under the microscope it exhibits a fragmental structure, the felspars are often somewhat rounded in outline, are frequently rather decomposed and speckled with minute flakes of white mica of secondary origin, and commonly seem to "tail off" into a sort of stream of microlithic mica, which has doubtless resulted from the destruction of felspar, the residual silica making its appearance as minutely crystalline quartz. The original quartz grains have been broken up, and are now represented by smaller grains, often in rudely lenticular aggregates, like little "inliers" of quartzite. The original flakes of black mica have been tattered and torn, and now appear as streaky clusters of flakelets, often less than one-sixth the original length. In extreme cases of crushing, the felspar has almost disappeared; the constituents are all reduced in size, and the rock at first sight would no longer be called a gneiss, but a fine-grained mica-schist. It has become extremely fissile, and the flat faces of the fragments exhibit a peculiar sheen, as if it had received a varnish of microlithic mica. In short, from a granitoid rock a microcrystalline mica-schist has been produced, which, however, differs markedly from the rock to which that name is ordinarily applied.

Let us now turn to a rock of similar nature, in which the effect of shearing is more conspicuous. I have selected a specimen, in which, as in the first example above, some of the felspar still remains in recognizable fragments. These, however, are commonly destitute of the "tail" of mica-microliths, and bear, at first sight, some resemblance to the broken porphyritic felspars which occur in a rhyolite. The mica, whether primary, but fragmental, or secondary, tends to get associated in undulating layers; the quartz also has a more uniform aspect and a more linear arrangement. In the most extreme cases the felspar all but disappears (though I fancy that it has here a better chance of surviving), the quartz and the mica are more and more aggregated in definite but thin bands, and the former, when viewed with crossing nicols, exhibits streaks, which, for a considerable distance, are almost uniform in tint, as if its molecules under a stress definite in direction had acquired a polarity, so that groups of these act upon light almost like a single crystal.

The effects of mechanical deformation, followed by mineral change, are also remarkably conspicuous in the case of pyroxenic rocks. Augite, it is well known, is by no means a stable mineral, and under certain circumstances is readily transformed into hornblende. This occurs in more than one way without mechanical action, but of these I do not now speak. Only of late years, however, has it been known that pressure can convert a dolerite into a hornblende-schist. Of this, through the kindness of Mr. Teall, who first proved the occurrence of this alteration in Great Britain, I can show you an example. The rock, as you see, has lost the structures of a dolerite, and has assumed those characteristic of many hornblende-schists. I say of many, because, though the rock is distinctly foliated, it does not exhibit a conspicuous mineral banding. My own observations confirm those of Mr. Teall, though I have never been so fortunate as to obtain, as he did, a complete demonstration of the passage from the one rock to the other.

It seems, then, to be demonstrated that, by mechanical deformation, accompanied or followed by molecular re-arrangement, foliated rocks, such as certain gneisses and certain schists, can be produced from rocks originally crystalline. But obviously there are limits to the amount of change. The old proverb, "You cannot make a silk purse of a sow's ear," holds good in this case also. To get certain results, you must have begun with rocks of a certain character. So that it is often possible, as I believe, to infer not only the nature of the change, but also that of the original rock. Hitherto we have been dealing with rocks which were approximately uniform in character, though composed of diverse materials—that is, with rocks more or less granular in aspect. Suppose, now, the original rock to have already acquired a definite structure—suppose it had assumed, never mind how, a distinct mineral banding, the layers varying in thickness from a small fraction of an inch upwards. Would this structure survive the mechanical deformation? I can give an answer which will at any rate carry us a certain way. I can prove that subsequent pressure has frequently failed to obliterate an earlier banded structure. In such a district as the Alps we commonly find banded gneisses and banded schists, which have been exposed to great pressure. Exactly as in the former case, the new divisional planes are indicated by a coating of films of mica, by which the fissility of the rock in this direction is increased. The mass has assumed a cleavage-foliation. I give it this name because it is due to the same cause as ordinary cleavage, but is accompanied by mineral change along the planes of division; while I term the older structure stratification-foliation, because so frequently, if it has not been determined by a stratification of the original constituents, it is at any rate a most extraordinary imitation of such an arrangement. In many cases the new structure is parallel with the old, but in others, as in the "strain-slip" cleavage of a phyllite, the newer can be seen distinctly cutting across the older mineral banding. As an example, take a rock mainly consisting of quartz and mica. Sometimes there has been a certain amount of crushing of the constituents, followed by a re-crystallization of the quartz and the formation of a pale-coloured mica. Sometimes, when the direction of the disturbance has been at right angles to the stratification-foliation, the latter is made wavy, and the mica-flakes are twisted round at right angles to their original position. Sometimes there has been a dragging or shearing of the mass, so that a considerable amount of mica has been re-crystallized along the new planes of division. To put it briefly, I assert, as the result of examining numbers of specimens, that though in certain cases the new structure is dominant, a practised eye seldom fails to detect traces of the older foliation, while in a large number of instances it is still as definite as the stripe in a slate.

We have got, then, thus far, that pressure acting on rocks previously crystallized can produce a foliation; but when it has acted in Palæozoic or later times, the resulting structures can be identified, and these, as a rule, are distinguishable from those of the most ancient foliated rocks, while at present we have found no proof that pressure alone can produce any conspicuous mineral banding. I am aware that this statement will be disputed, but I venture to plead, as one excuse for my temerity, that probably few persons in Great Britain have seen more of crystalline rocks, both in the field and with the microscope, than myself. So, while I do not deny the possibility of a well-banded rock being due to pressure alone, I unhesitatingly affirm that this at present is a mere hypothesis—an hypothesis, moreover, which is attended by serious difficulties. For, if we concede that, in the case of many rocks originally granular, dynamic metamorphism has produced a mineral banding, this is only on a very small scale: the layers are but a small fraction of an inch thick. No one

could for a moment confuse a sheared granite from the Highlands with a Laurentian gneiss from Canada or with an uninjured Hebridean gneiss. For the former to attain to the condition of the latter, the mass must have been brought to a condition which admitted of great freedom of motion amongst the particles, almost as much, in short, as among those of a molten rock. Clearly, the dynamic metamorphism of Palæozoic or later ages appears to require some supplementary agency. Can we obtain any clue to it?

An explanation of broadly-banded structures was long since suggested, and has recently been urged with additional force, which avoids some of our difficulties. We know that the process of consolidation in a coarsely crystalline rock has often been a slow one; the constituent minerals separate gradually from the magma, of which sometimes so little may remain, that a rock with a true glassy base has been mistaken for one holocrystalline. The residual and still unsolidified magma would admit of a slow flowing of the mass, but there would be so little of it that the crystals already individualized, though altered in position by differential movements, would be affected by strains, and liable to fracture. Such a rock, when finally consolidated, would exhibit many phenomena in common with a rock modified by dynamic metamorphism, but would differ in the greater coarseness of its structure. This may prove to be the correct explanation of the curious foliated and banded gabbros in the Lizard district. That some crystalline rocks must have passed through this stage I am now in a position to affirm, from evidence not yet published.

Let us, however, see whether another line of investigation may not throw some light on our difficulty. I have already mentioned the effect produced by the intrusion of large masses of igneous rocks upon other rocks. These may be either igneous rocks already solidified, or sedimentary rocks. The former may be passed over, as they will not materially help us. In regard to the latter, the results of contact-metamorphism, as it is called, are, as we might expect, very various. Speaking only of the more extreme, we find that sandstones are converted into quartzites; limestones become coarsely crystalline, all traces of organisms disappearing, and crystalline silicates being formed. In clayey rocks all signs of the original sediments disappear, crystalline silicates are formed, such as mica (especially brown), garnet, andalusite, and sometimes tourmaline; felspar, however, is very rare. Fair-sized grains of quartz appear, either by enlargement of original granules or by independent crystallization of the residual silica. It is further important to notice that, as we approach the surface of the intrusive mass—that is, as we enter upon the region where the highest temperature has been longest maintained—the secondary minerals attain a larger size and are more free from adventitious substances—that is, they have not been obliged as they formed to incorporate pre-existing constituents. The rock, indeed, has not been melted down, but it has attained a condition where a rather free molecular movement became possible, and a new mineral in crystallizing could, as it were, elbow out of the way the more refractory particles. I can, perhaps, best bring home to you the result of contact-metamorphism by showing you what its effects are on a rock similar to that which I exhibited in illustration of the effect of pressure-metamorphism on a distinctly stratified rock. These are, in brief, to consolidate the rock, and while causing some constituents to vanish, to increase greatly the size of all the others. It follows, then, that mineral segregation is promoted by the maintenance for some time of a high temperature, which is almost a truism. I may add to this that, though rocks modified by contact-metamorphism differ from the Archæan schists, we find in them the best imitations of stratification-foliation, and of other structures characteristic of the latter.

One other group of facts requires notice before we proceed to draw our inferences from the preceding. Very commonly, when a stratified mass rests upon considerably older rocks, the lower part of the former is full of fragments of the latter. Let us restrict ourselves to basement beds of the Cambrian and Ordovician—the first two chapters in the stone-book of life. What can we learn from the material of their pages? They tell us that granitoid rocks, crystalline schists of various kinds, as well as quartzites and phyllites, then abounded in the world. The Torridon sandstone of Scotland proves that much of the subjacent Hebridean had even then acquired its present characteristics. The Cambrian rocks of North and South Wales repeat the story, notably near Llynfaelog in Anglesey, where the adjacent gneissoid rocks from which the pebbles were derived, even if once true granites, had assumed their differences before the end of the Cambrian period. By the same time similar changes had affected the crystalline rocks of the Malverns and parts of Shropshire. It would be easy to quote other instances, but these may suffice. I will only add that the frequent abundance of slightly-altered rocks in these conglomerates and grits appears significant. Such rocks seem to have been more widely distributed—less local—than they have been in later periods. Another curious piece of evidence points the same way. In North America, as is well known, there is an ancient group of rocks to which Sir W. Logan gave the name Huronian, because it was most typically developed in the vicinity of Lake Huron. Gradually great confusion arose as to what this term really designated. But now, thanks to our fellow-workers on the other side of the Atlantic, the fogs, gendered in the laboratory, are being dispelled by the light of microscopic research and the fresh air of the field. We now know that the Huronian group in no case consists of very highly-altered rocks, though some of its members are rather more changed than is usual with the British Cambrians, than which they are supposed to be slightly older. Conglomerates are not rare in the Huronian. Some of these consist of granitoid fragments in a quartzose matrix. We cannot doubt that the rock was once a pebbly sandstone. Still the matrix, when examined with the microscope, differs from any Palæozoic sandstone or quartzite that I have yet seen. Among grains of quartz and felspar are scattered numerous flakes of mica, brown or white. The form of these is so regular that I conclude they have been developed, or at least completed, *in situ*. Moreover, the quartz and the felspar no longer retain the distinctly fragmental character usual in a Palæozoic grit, but appear to have received secondary enlargement. A rock of fragmental origin to some extent has simulated or reverted to a truly crystalline structure. In regard to the larger fragments we can affirm that they were once granitoid rock, but in them also we note incipient changes such as the development of quartz and mica from felspar (without any indication of pressure), and there is reason to think that these changes were anterior to the formation of the pebbles.

To sum up the evidence. In the oldest gneissoid rocks we find structures different from those of granite, but bearing some resemblance to, though on a larger scale than, the structures of vein-granites or the surfaces of larger masses when intrusive in sedimentary deposits. We find that pressure alone does not produce structures like these in crystalline rocks, and that when it gives rise to mineral banding this is only on a comparatively minute scale. We find that pressures acting upon ordinary sediments in Palæozoic or later times do not produce more than colourable imitations of crystalline schists. We find that when they act upon the latter the result differs, and is generally distinguishable from stratification-foliation. We see that elevation of temperature obviously facilitates changes, and promotes coarseness of structure. We see also that the rocks in a crystalline series which appear to occupy the highest position seem to be the least metamorphosed, and present the strongest resemblance to stratified rocks. Lastly, we see that mineral change appears to have taken place more readily in the later Archæan times than it ever did afterwards. It seems, then, a legitimate induction that in Archæan times conditions favourable to mineral change and molecular movement—in short, to metamorphism—were general, which in later ages have become rare and local, so that, as a rule, these gneisses and schists represent the foundation-stones of the earth's crust.

On the other side what evidence can be offered? In the first place, any number of vague or rash assertions. So many of these have already come to an untimely end, and I have spent so much time and money in attending their executions, that I do not mean to trouble about any more till its advocates express themselves willing to let the question stand or fall on that issue. Next, the statement of some of the ablest men among the founders of our science, that foliation is more nearly connected with cleavage than with structures suggestive of stratification. In regard to this I have already admitted, in the case of the more coarsely crystalline rocks, what is practically identical with their claim, for they also assert that when the banding was produced, very free movement of the constituents was possible; and in regard to the rest I must ask whether they were speaking of cleavage-foliation or stratification-foliation, which had not then been distinguished, and I know in some instances what the answer will be. The third objection is of a general nature. To

prevent the possibility of misstatement I will give it as a quotation:—"To a geologist (especially one belonging to the school of Lyell) it is equally difficult to conceive that there should be a broad distinction between the metamorphic rocks of Archæan and post-Archæan age respectively, as that the pre-Tertiary volcanic rocks should be altogether different in character from those of Tertiary and recent times." Of course in this statement much depends on the sense attached to the epithet "broad." As an abstract proposition I should admit, as a matter of course, that from similar causes similar consequences would always follow. But in the latter part of the quotation lurks a *petitio principii*. During the periods mentioned volcanic rocks appear, as we should expect, to have been ejected from beneath the earth's crust similar in composition and condition, and to have solidified with identical environment. Hence the results, allowing for secondary changes, should still be similar. But to assume that the environment of a rock in early Archæan times was identical with that of similar material at a much later period is to beg the whole question. My creed, also, is the uniformitarian; but this does not bind me to follow a formula into a position which is untenable. Other studies with which I have some familiarity have warned me that a blind orthodoxy is one of the best guides to heresy. "The weakness and the logical defect of uniformitarianism"—these are Prof. Huxley's words—"is a refusal, or at least a reluctance, to look beyond the 'present order of things,' and the being content for all time to regard the oldest fossiliferous rocks as the *Ultima Thule* of our science." Now, speaking for myself, I see no evidence since the time of these rocks, as at present known, of any very material difference in the condition of things on the earth's surface. The relations of sea and land, the climate of regions, have been altered; but because I decline to revel in extemporized catastrophes, and because I believe that in Nature order has prevailed and law has ruled, am I therefore to stop my inquiries where life is no longer found, and we seem approaching the firstfruits of the creative power? Because palæontology is, perforce, silent; because the geologist can only say, "I know no more," must I close my ear to those who would turn the light of other sciences upon the dark places of our own, and meet their reasoning with the exclamation, "This is not written in the book of uniformity"? To do this would be to imitate the silversmiths of old, and silence the teacher by the cry, "Great is Diana of the Ephesians."

What, then, does the physicist tell us was the initial condition of this globe? I will not go into the vexed question of geological time, though as a geologist I must say that we have reason to complain of Sir W. Thomson. Years ago he reduced our credit at the bank of time to a hundred millions of years. We grumbled, but submitted, and endeavoured to diminish our drafts. Now he has suddenly put up the shutters, and declared a dividend of less than four shillings in the pound. I trust some aggrieved shareholder will prosecute the manager. However, as a *cause célèbre* is too long a business for the end of an evening, I will merely say that, while personally I see little hope of arriving at a chronological scale for the age of this earth, I do not believe in its eternity. What, then, does the physicist tell us must have been in the beginning? I pass by those earliest ages, when, as "Ilion, like a mist, rose into towers," so from the glowing cloud the great globe was formed. I pass on to a condition more readily apprehended by our faculties—the time, the *consistentior status* of Leibnitz, when the molten globe had crusted over, and its present history began. Rigid uniformitarian though you may be, you cannot deny that when the very surface of the ground was at a temperature of at least 1000° F., there was no rain, save of glowing ashes—no river, save of molten fire. Now is ending a long history with which the uniformitarian must not reckon—of a time when many compounds now existing were not dissolved but dissociated, for combination under that environment was impossible. Yet there was still law and still order—nay, the present law and order may be said even then to have had a potential existence—nevertheless to the uniformitarian gnome, had such there been, every new combination of elements would have been a new shock to his faith, a new miracle in the earth's history. But at the times mentioned above, though oxygen and hydrogen could combine, water could not yet rest upon the ruddy crust of the globe. What does that mean? This, that assuming the water of the ocean equivalent to a spherical shell of the earth's radius and two miles thick, the very lava-stream would consolidate under a pressure of about 310 atmospheres, equivalent to nearly 4000 feet of average rock.[1] But on the practical bearing of this consideration I will not dwell. Let us pass on to a time which, according to Sir W. Thomson, would rather quickly arrive, when the surface of the crust had cooled by radiation to its present temperature. Let us, merely for illustration, take a surface temperature of 50° F. (nearly that of London), and assume that the present rise of crust temperature is 1° F. for every 50 feet of descent, which is rather too rapid. If so, 212° F. is reached at 8100 feet, and 250° F. at 10,000 feet. Though the latter temperature is far from high, yet we should expect that under such a pressure chemical changes would occur with much more facility than at the surface. But many Palæozoic or even later rock masses can now be examined which at a former period of their history have been buried beneath at least 10,000 feet of sediment; yet the alteration of their constituents has been small: only the more unstable minerals have been somewhat modified, the more refractory are unaffected. But for a limited period after the *consistentior status*, the increase of crust temperature in descending would be far more rapid; when one-twenty-fifth of the whole period from that epoch to the present had elapsed, and this is no inconsiderable fraction, the rate of increase would be 1° for every 10 feet of descent. Suppose, for the sake of comparison, the surface temperature as before, the boiling-point of water would be reached at 1620 feet, and at 10,000 feet, instead of a temperature of 250° F., we should have one of 1050° F. But at the latter temperature many rock masses would not be perfectly solid.[2] According to Sorby, the steam cavities in the Ponza trachyte must have formed, and thus the rock have been still plastic at so low a temperature as 680° F. At this period, then, the end of the fourth year of the geological century, whatever be its units, structural changes in igneous and chemical changes in sedimentary rocks must have occurred more readily than in any much later period in the world's history. A temperature of 2000° F., sufficient to melt silver—more than sufficient to melt many lavas—would have been reached at a depth of about 4 miles. It would now be necessary to descend for at least 20 miles in order to arrive at this zone. It, during the ninety-six years of the century, has been changing its position in the earth's crust, more slowly as time went on, from the one level to the other.

There is another consideration, too complicated for full discussion, too uncertain, perhaps, in its numerical results to be more than mentioned at present, which, however, seems to me important. It is this, that in very early times, as shown by Prof. Darwin and Mr. Davison, the zone in the earth's crust, at which lateral thrust ceases and tension begins, must have been situated much nearer to the surface than at present. If now, at the end of the century, it is at the depth of 5 miles, it was, at the end of the fourth year, at a depth of only 1 mile. Then, a mass of rock, 10,000 feet below the surface, would be nearly a mile deep in the zone of tension. Possibly this may explain the mineral banding of much of our older granitoid rock, already mentioned, and the coincidence of foliation with what appears to be stratification in the later Archæan schists, as well as the certainly common coincidence of microfoliation with bedding in the oldest indubitable sediments.

Pressure, no doubt, has always been a most important factor in the metamorphism of rocks; but there is, I think, at present some danger in over-estimating this, and representing a partial statement of truth as the whole truth. Geology, like many human beings, suffered from convulsions in its infancy; now, in its later years, I apprehend an attack of pressure on the brain.

The first deposits on the solidified crust of the earth would obviously be igneous. As water condensed, denudation would begin, and stratified deposits, mechanical and chemical, become possible, in addition to detrital volcanic material. But at that time the crust itself, and even stratified deposits, would often be kept for a considerable period at a temperature similar to that afterwards produced by the invasion of an intrusive mass. Thus not only rocks of igneous origin (including volcanic ashes) would predominate in the lowest foundation-stones, but also secondary changes would occur more readily, and even the sediments or precipitates should be greatly metamorphosed. Strains set up by a falling temperature would produce, in masses still plastic, banded structures, which, under the peculiar circumstances,

[1] If we take the specific gravity of water as unity, and that of mean rock as 2·7, the pressure would be = 3911·1 feet of rock.

[2] The lowest temperature, which, so far as I know, has been observed in lava (basic) while still plastic, is 1228° F.

might occur in rocks now coarsely crystalline. As time went on, true sediments would predominate over extravasated materials, and these would be less and less affected by chemical changes, and would more and more retain their original character. Thus we should expect that as we retraced the earth's course through "the corridor of time," we should arrive at rocks which, though crystalline in structure, were evidently in great part sedimentary in origin, and should beyond them find rocks of more coarsely-crystalline texture and more dubious character, which, however, probably were in part of a like origin; and should at last reach coarsely-crystalline rocks, in which, while occasional sediments would be possible, the majority were originally igneous, though modified at a very early period of their history. This corresponds with what we find in Nature, when we apply, cautiously and tentatively, the principles of interpretation which guide us in stratigraphical geology.

I have stated as briefly as possible what I believe to be facts. I have endeavoured to treat these in accordance with the principles of inductive reasoning. I have deliberately abstained from invoking the aid of "deluges of water, floods of fire, boiling oceans, caustic rains, or acid-laden atmospheres," not because I hold it impossible that these can have occurred, but because I think this epoch in the earth's history so remote and so unlike those which followed, that it is wiser to pass it by for the present. But unless we deny that any rocks formed anterior to or coæval with the first beginning of life on the globe can be preserved to the present time, or, at least, be capable of identification (an assumption which seems to me gratuitous and unphilosophical) then I do not see how we can avoid the conclusion to which we are led by a study of the foundation-stones of the earth's crust—namely, that these were formed under conditions and modified by environments which, during later geological epochs, must have been of very exceptional occurrence. If, then, this conclusion accords with the results at which students of chemistry and students of physics have independently arrived, I do not think that we are justified in refusing to accept them, because they lack the attractive brilliancy of this or that hypothesis, or do not accord with the words in which a principle, sound in its essence, has been formulated. It is true in science, as in a yet more sacred thing, that "the letter killeth, the spirit giveth life."

SYSTEMATIC RELATIONS OF PLATYPSYLLUS AS DETERMINED BY THE LARVA.

PROF. C. V. RILEY, in a paper read at a recent meeting of the National Academy of Science (U.S.A.), drew attention to the unique character of *Platypsyllus castoris*, a parasite of the beaver; and gave an epitome of the literature on the subject, showing how the insect had puzzled systematists, and had been placed by high authority among the Coleoptera and the Mallophaga, and made the type even of a new order. He showed the value, as at once settling the question of its true position, of a knowledge of the adolescent states. He had had since November 1886 some 14 specimens of the larva, obtained from a beaver near West Point, Nebraska, and had recently been led to study his material at the instance of Dr. Geo. H. Horn, of Philadelphia, who at a recent meeting of the Entomological Society of Washington announced the discovery of the larva by one of his correspondents the present spring, and will publish a full description of it. Prof. Riley indicated the undoubted Coleopterological characteristics of the insect in the imago state, laying stress on the large scutellum and five-jointed tarsi, which at once remove it from the Mallophaga, none of which possess these characters. He also showed that the larva fully corroborates its Coleopterological position, and that its general structure, and particularly the trophi, anal cerci, and pseudopod, confirm its Clavicorn affinities. He showed that the atrophied mandibles in the imago really existed as described by Le Conte, and that even in the larva they were feeble and of doubtful service in mastication. He mentioned, as confirmatory of these conclusions, the finding by one of his agents, Mr. A. Koebele, of Leptinillus (the Coleopterological nature of which no one has doubted, and the nearest ally to Platypsyllus), associated with Platypsyllus upon beaver-skins from Alaska; also the parasitism of Leptinus upon mice. He paid a high compliment to the judgment and deep knowledge of the late Dr. Le Conte, whose work on the imago deserves the highest praise, and whose conclusions were thus vindicated. "Platypsyllus therefore," he concluded, "is a good Coleopteron, and in all the characters in which it so strongly approaches the Mallophaga it offers merely an illustration of modification due to food, habit, and environment. In this particular it is, however, of very great interest as one of the most striking illustrations we have of variation in similar lines through the influence of purely external or dynamical conditions, and where genetic connection and heredity play no part whatever. It is at the same time interesting because of its synthetic characteristics, being evidently an ancient type, from which we get a very good idea of the connection in the past of some of the present well-defined orders of insects."

SCIENTIFIC SERIALS.

Atti della R. Accademia dei Lincei, July and August 1888.—In both of these numbers G. Vicentini and D. Omodei continue their important inquiries on the thermic expansion of certain binary alloys in the liquid state. So far they have arrived at the following general conclusions: (1) the variation of volume accompanying liquid metallic mixtures is extremely slight; (2) no relation can be established between the variations of volume that accompany the formation of alloys in the solid and liquid states; (3) the variation of density at the moment of solidification is in general less than would be the case were the constituent metals to preserve in the alloys the value that they possess in the isolated state; (4) the binary alloys of lead and tin, of tin and bismuth, and of tin and cadmium, possess in the state of perfect fusion an expansion equal to that resulting from the sum of the expansions of the associated metals; (5) the alloy of Bi_2Pb possesses a coefficient of expansion far greater than the sum of the expansions of the constituent metals. These experiments, which conclude for the present with a preliminary study of the antimony and zinc alloys, have been carried out at the physical laboratory of the University of Cagliari, Sardinia.

Rivista Scientifico-Industriale, October.—Experiments made with Crookes's radiometer, by Prof. Pietro Lancetta. The experiments here described have been undertaken chiefly for the purpose of making a synthesis of certain phenomena which are more easily produced by this apparatus than by any other means. It is also shown that the radiometer may in some cases be more advantageously employed than the ordinary thermometer, especially in testing certain laws regarding latent and luminous heat, Crookes's instrument being sensible both to the dark and luminous wave of the solar rays. The results of the experiments show generally that in a homogeneous medium the radiation of the thermo-luminous wave is propagated in a straight line; that the luminous wave is propagated *in vacuo*; that the intensity of the thermo-luminous wave is in inverse ratio to the square of the distance; that the evaporation of fluids as well as the rarefaction of gaseous bodies is accompanied by a lowering of the temperature, while the condensation of gas develops heat.

Journal of the Russian Chemical and Physical Society, vol. xx. fasc. 6.—On the speed and the products of decomposition of the chlorate and chlorite of lithium, by A. Potilitzin, being the second part of an inquiry into the properties of galloid compounds. The decomposition of the two above-mentioned salts, as well as of the bromate of barium, is best explained according to the law of unstable equilibrium indicated by the author in his former works, and which he sums up as follows: in each chemical reaction the equilibrium of the system depends upon the values of their atomic weights, their masses, and their stock of potential energy.—On the relation between the rotatory power and the refraction of organic compounds, by J. Kanonnikoff, first part.—On the action of organic iodides on natrium-nitro-ethane, by N. Sokoloff.—Obituary notice of Prof. Wroblewski, by S. Lamansky.—The total eclipse of the sun of August 19, 1887, by N. Egoroff; and on the results of meteorological observations during the same eclipse, by N. Hesehus.

SOCIETIES AND ACADEMIES.

LONDON.

Royal Society, May 31.—"On the Effect of Occluded Gases on the Thermo-electric Properties of Bodies, and on their Resistances; also on the Thermo-electric and other Properties of Graphite and Carbon." By James Monckman, D.Sc. Communicated by Prof. J. J. Thomson, F.R.S.

A piece of platinum wire about 18 inches long was bent in the middle, and one-half protected by being covered with glass tube and made water-tight at the lower end. After annealing the free portion and testing until perfectly free from all strain effects, it was placed, up to about the middle, in acidulated water, and made the negative pole of a battery, and hydrogen liberated upon it for a few minutes. After being dried it was tested with a small flame at distances 1 cm. along its whole length. The result was a current from free wire towards that part on which hydrogen had been produced, greatest at the junction of the free wire and the saturated wire.

When wires of palladium were used, more powerful effects of the same kind were produced.

Carbon rods were next tried. It was found that when one of these rods was heated and placed against the other, the current was always from cold to hot below 200° C.

They were then used as the electrodes in decomposing dilute sulphuric acid, dried carefully until no current was produced on placing them in contact. On heating either rod and joining them as before, a current was produced from hydrogen to oxygen across the hot junction.

The same effect was obtained by decomposing hydrochloric acid solution.

Resistance.—To get rid of possible error from change of temperature, two wires of equal length and section were used and balanced against each other.

These were placed in water, and a current passed from the one to the other, allowed to remain in the acid a little to cool if necessary, and afterwards removed, dried, and placed in an empty glass vessel surrounded with a considerable quantity of water. There they rested until the temperature became the same as the water. When measured, the resistance of the wire containing the hydrogen was found to have increased about one-thousandth part.

Carbon.—Two thin rods about 2mm. diameter were electroplated at the ends and soldered to insulated copper wires.

When used as the poles of a battery the change of resistance was considerable, but greater on the rod that had been the positive pole. By using a platinum electrode, hydrogen or oxygen was produced at will upon the same rod, the other rod remaining unchanged. It then appeared that oxygen increased the resistance much more than hydrogen, rising in some cases as high as nine times; that when oxygen was liberated twice or thrice in succession the resistance increased each time. This continued increase was probably due to chemical changes produced by the active oxygen. Hydrogen gave an increase of resistance, not continuing beyond a certain point, and not becoming greater on repeated charging with the gas.

Generally also the effect of the hydrogen was temporary, disappearing, wholly in some cases, partially in others, when short circuited.

Superposition of Polarization.—Part of the change in the carbon is evidently produced by the mechanical action of the gases evolved, and by the chemical action of the oxygen; both of these will, however, produce permanent changes. That only part of the action is to be explained in this way is shown by the previous experiments. It is, however, further demonstrated by using two carbon rods in decomposing acidulated water; after passing the current for one minute, reverse it for one-tenth of a second and immediately join up to a galvanometer. A short but violent deflection appears for the latter contact, gradually falling to zero and passing to the other side, where it remains for a considerable time, though with much decreased quantity.

The same thing was obtained with platinum electrodes. The second contact must be very short, or the former polarization disappears.

Thermo-electric and other Properties of Graphite and Carbon.—In making the previous experiments, I had occasion to place the heated end of one carbon rod in contact with the cold end of another. The temperature of the hot end was varied from 30° C. to a red heat, whilst the cold end was kept at about 17° C.

Currents of electricity were of course produced. When the temperature of the hotter rod was raised but slightly, the current was from cold to hot through the point of contact, but when it was raised to a red heat the current passed from hot to cold.

I was led to expect that the line of carbon in a thermo-electric diagram, in which the area of the space between the lines is proportional to the electromotive force, would show a bend of some kind, and as no researches were known showing such a bend, it appeared desirable to test it carefully.

Near one end of a carbon rod a hole, about 5 mm. in diameter, was drilled, and into this the end of a platinum wire was inserted and fixed by being wedged with a piece of rod carbon. The whole was thoroughly covered with Indian ink, which, when dry, was again covered with clay. The carbon rod was insulated from the platinum wires, and they from each other by thin sheet asbestos and mica, by which means it was insulated from the vessel in which it was placed, and luted with clay to prevent access of air. From several series of experiments a thermo-electric line was calculated, and found to rise from 0° to 250°, beyond which it descended at the same rate.

Other Changes in the Properties of the Body at the same Temperature.

This change in the thermo electric power of carbon is accompanied by other changes. The resistance, the expansion, and the specific heat all appear to undergo a corresponding alteration, as the following summary of results shows:—

	Below 250° C.	Above 250° C.
A. Effect of contact of hot and cold carbon.	Current from cold to hot.	Current from hot to cold.
B. Thermo-electric line	Rises.	Falls.
C. Rate of decrease of resistance per degree per ohm	Diminishes.	Increases.
D. The rate of increase of the coefficient of expansion	Increases.	Decreases.
E. Rate of increase of the specific heat	Fairly regular.	Falls to half.

Geological Society.—November 7.—Dr. W. T. Blanford, F.R.S., President, in the chair.—The following communications were read:—The Permian rocks of the Leicestershire coal-field, by Horace T. Brown. According to Mr. Brown, the Permian rocks of the Leicestershire coal-field belong to the same area of deposition as those of Warwickshire and South Staffordshire, all having formed part of the detrital deposits of the Permian lake which extended northwards from Warwickshire and Worcestershire, and which had the Pennine chain on its eastern margin. He pointed out the dissimilar nature of these deposits to those of the eastern side of the Pennine chain from Nottingham to the coast of Durham. There were proofs of the existence of a land barrier, owing to the uprising of the Carboniferous, between the district round Nottingham and the Leicestershire coal-field. The most northerly exposure of the Leicestershire Permians is thirteen miles south-west of those of South Notts. He indicated the probable course of the old coast-line of the western Permian lake. Denudation had bared some of the older Palæozoics of their overlying Coal-measures, and it is the rearranged talus from the harder portions of these older rocks which now forms the brecciated bands in the Leicestershire Permians. The reading of the paper was followed by a discussion, in which the President, Prof. Bonney, Mr. Whitaker, Prof. Blake, and Mr. Topley took part.—On the superficial geology of the central plateau of North-Western Canada, by J. B. Tyrrell, Field Geologist of the Geological and Natural History Survey of Canada. The drift-covered prairie extends from the west side of the Lake of the Woods to the region at the foot of the Rocky Mountains, rising from a height of 800 feet on the east to 4500 feet on the west, the gentle slope being broken by two sharp inclines known as the Pembina Escarpment and the Missouri Coteau, giving rise to the First, Second, and Third Praire Steppes. The author described the older rocks of this region, referring especially to his subdivision of the Laramie formation into an Edmonton series of Cretaceous age, and a Pascapoo series forming the base of the Eocene, and then discussed the superficial deposits in the following order: (1) *Preglacial gravels;* occurring along the foot of the Rocky Mountains, composed of waterworn quartzite pebbles, similar to those now forming, and, like them, produced by streams flowing from the mountains; (2) *Boulder-clay* or *Till;* (3) *Interglacial deposits* of stratified material; (4) *Moraines;* (5) The *Kames* or *Åsar*, generally occurring at the bottoms of wide valleys; (6) *Stratified deposits* and beach-ridges which have been formed at the bottoms and along the margins of fresh-water lakes lying along the foot of the ice-sheet; (7) *Old Drainage-channels.* Some remarks on this paper were made by the President, Dr. Hinde, Mr. Whitaker, Mr. Topley, and Mr. Marr.

PARIS.

Academy of Sciences, November 12.—M. Janssen in the chair.—On the cultivation of the square-eared variety of wheat in 1887 and 1888, by MM. E. Porion and P. P. Dehérain. The results of two years' experience in various parts of France show that this variety of red wheat yields under favourable conditions crops which till lately would have been thought fabulous. It succeeds best on well-drained heavy clay soils in the central and northern provinces, and if well manured and sown in regular furrows gives splendid returns; it also shows more power of resisting the destructive action of wind and rain than any other kind.—On the nature of milk, by M. A. Béchamp. This is a reply to the question, Does milk contain any anatomical elements of the system, and if so, do the lacteous globules represent any of these elements? The author's studies lead to the conclusion that milk is not an emulsion; that the lacteous globules are real adipose vesicles in a free state, and that cows' milk contains, besides caseine, other albuminoid substances, not in a free state, but combined in solution with alkalies.—Calculation of the tensions of sundry vapours, by M. Ch. Antoine. Here the general formula $\log p = A\left(D - \frac{1000}{\theta}\right)$ is applied to a number of vapours such as benzene, chloroform, alcohol, carbon chloride, ether, acetone, and carbonic acid, and the comparative results are given for the five temperatures that served as the base for the calculation of Regnault's new formula. The final formulas thus obtained, which had never been worked out by Regnault, must henceforth be adopted in studying the properties of the vapour of acetone.—A new method of improving the capacity of very long telegraphic lines, by M. Fernand Godfroy. This method consists in establishing at each extremity of the line a connection with earth, having a coefficient of self-induction powerful enough, if not to compensate the usual waste, at least greatly to diminish it by the inverse effects which the self-induction tends to produce. The method has already been tried with marked success, on several underground lines, amongst others that between the Paris central station and Angoulême, a distance of 300 miles. Here it was found possible to signal at the rate of twenty words per minute with an *ordinary* Morse apparatus, without any intermediate or local translator, and utilizing one direction only of the current.—On the silicated combinations of glucine, by MM. P. Hautefeuille and A. Perrey. The elements of a leucite or volcanic schorl with aluminia or glucine base ($4SiO_2,Al_2O_3,KO$, or $4SiO_2,Gl_2O_3,KO$), heated between 600° and 800° C. with an excess of neutral vanadate of potassa, are rapidly mineralized. But the nature of the resulting crystals varies in the course of the same operation according as the mineralizing agent yields to the product by which an increasing portion of its alkali is crystallized. Thus this product is homogeneous only under exceptional conditions, and as a rule is a mixture of several chemical species, whose separation is here studied.—Presence of glycolic acid and of normal propylenedicarbonic acid in the grease of sheep's wool, by MM. A. and F. Buisne. In the process of analyzing sheep-washings, the authors have succeeded in isolating these two acids, which are here described. The latter, with formula $COOH—CH_2—CH_2—CH_2—COOH$, is shown to be a higher homologue of succinic acid.—On the Hersiliidæ, a new family of parasitic Copepods, by M. Eugène Canu. Thanks to his discovery at Wimereux of two new genera closely allied to the Hersilia, and parasites of various invertebrates, the author has come to the conclusion that the Hersilia should constitute a new family as distinct from the Siphonostoms as they are from the Peltidians. A full anatomical description is given of this family of Hersiliidæ.—On a new geological map of France, by MM. Jacquot and Michel Lévy. This map, which is on the scale of 1 : 1,000,000, will be issued by the French Geological Service before the close of the year, and will embody the latest researches, including the unpublished reports for 1884–86.—M. du Chatellier has a note on the continued subsidence of the Finistère coast, Brittany; and M. V. Galtier describes some fresh experiments tending to show the efficacy of intra-venous injections of the virus of rabies as a prophylactic against the bite of mad dogs.

Astronomical Society, November 3.—M. C. Flammarion, President, in the chair.—M. Riccò sent a description of a sun-spot observed from November 12 to 25, 1882. This spot, the largest ever observed, had an area over fifty times that of a great circle of the earth. It exhibited rose-coloured veils in some parts, and the nuclei were crossed by large yellow arcs.—M. Gaudibert sent a drawing of Eratosthenes, showing twelve hills within the ring, and a multitude of small craters on the east.—M. Blot, of Clermont (Oise), sent an elementary demonstration of the formula $v^2 = \mu\left(\frac{2}{R} - \frac{1}{a}\right)$, which gives the velocity of a planet in terms of the major axis and of the radius-vector.—Observations of meteors, by M. de Alcantara Penya at Majorca, on June 23, at 7.20 p.m.; by Baroness Ottenfels in the Gulf of Juan, on July 17, at 9 p.m.; by M. Henrionnet at Troyes, on September 9, at 9.30 p.m.—Observations of sun-spots, by MM. Bruguière, Henrionnet, and Loiseau; and of Sawerthal's comet, by MM. Guillaume and Kropp.—M. Guiot, of Soissons, saw the companion to Sirius with a 3¾-inch refractor, and observed Vesta with the naked eye from September 5 to 20.—M. Ferret photographed the moon with a 3¾-inch refractor.—M. Duprat sent an observation of the lunar eclipse of July 23 made at Constantine.—M. Lihou observed an occultation of a seventh magnitude star by Jupiter.—M. Foray observed Venus on the day of its conjunction, with a 4-inch refractor.—The Sociétés Scientifiques Flammarion, of Marseilles, Argentan, Bruxelles, Jaën, and Bogota, were elected Corresponding Societies.

BOOKS, PAMPHLETS, and SERIALS RECEIVED.

The Orchids of the Cape Peninsula: H. Bolus (Cape Town).—Monographs of the United States Geological Survey, vol. xii. (Washington).—Atlas to accompany a Monograph on the Geology and Mining Industry of Leadville, Colorado: S. F. Emmons (Washington).—Report of the Meteorological Service of the Dominion of Canada for the year ending December 31, 1885 (Ottawa).—The Kingdom of Georgia: O. Wardrop (Low).—New Zealand of To-day: J. Bradshaw (Low).—Physical Realism: T. Case (Longmans).—Cours de Thermodynamique: M. Lippmann (Paris, Georges Carre).—Truth for its own Sake—the Story of Charles Darwin: W. Mawer (Sonnenschein).—Travaux et Mémoires du Bureau International des Poids et Mesures, Tome vi. (Paris, Gauthier-Villars).—Birds in Nature: R. B. Sharpe (Low).—Orient Line Guide, 3rd edition: edited by W. J. Loftie (Low).—The Least of All Lands: W. Miller (Blackie).—Bulletin de la Société Astronomique de France, Première Année, 1887 (Paris).—Transactions and Proceedings of the Royal Society of Victoria, vol. xxiv. Part 2 (Williams and Norgate).—Bolletino della Società di Naturalisti in Napoli, Serie ii., vol. ii., Anno 2, Fasc. 2 (Napoli).

CONTENTS.

PAGE
The Opening of the Pasteur Institute
Practical Botany
The Senses, Instincts, and Intelligence of Animals. By Prof. George J. Romanes, F.R.S.
Massage
Our Book Shelf:—
Rutley: "Rock-forming Minerals"
Hall and Stevens: "A Text-book of Euclid's Elements for the Use of Schools"
Fisher: "A Class-book of Elementary Chemistry"
Letters to the Editor:—
Alpine Haze. (*Illustrated.*)—**Antoine d'Abbadie**
Rankine's Modification of Newton's Investigation of the Velocity of Sound in any Substance.—**Prof. Oliver J. Lodge, F.R.S.**
A Simple Dynamo. (*Illustrated.*)—**Frederick J. Smith**
The Use of Rotifera.—**C. L.**
On the Mechanical Conditions of a Swarm of Meteorites. I. By Prof. G. H. Darwin, F.R.S.
Some Curious Properties of Metals and Alloys. By Prof. W. Chandler Roberts-Austen, F.R.S.
The Leonid Meteor-Shower, 1888. By W. F. Denning
Notes
Our Astronomical Column:—
The Brazilian Transit of Venus Expeditions, 1882
The Tail of Comet 1887 *a* (Thome)
Astronomical Phenomena for the Week 1888 November 25—December 1
Geographical Notes
The Foundation-Stones of the Earth's Crust. By Prof. T. G. Bonney, F.R.S.
Systematic Relations of Platypsyllus as determined by the Larva. By Prof. C. V. Riley
Scientific Serials
Societies and Academies
Books, Pamphlets, and Serials Received